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THE
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JULY, 1871.

Analytical and Critical Reviews.

I.—A Century of Therapeutics.¹

THERE is no greater wonder in the progress of science than the change that has taken place during the last hundred years in the matter of therapeutics. We have headed the list of books in our foot-note with the name of a work which, in its day, was the standard of cunning and wise “*formulæ medicamentorum*.” Edward Fox, the author of this work, was a man of mark in his day; he was apothecary to a royal lady; he was a fair writer, a friend of the leading physicians of his time, and an accepted authority in his own department. He dedicates his volume to Dr. Warren, F.R.S., Physician in Ordinary to King George the Third, convinced, he says, “that a work so dedicated will not be sent into the world under the disadvantage of any prejudices against it, and hoping that being thus introduced it may prove useful at least to those who, being placed at a distance from the metropolis, have few opportunities of seeing the present mode of medical practice.”

¹ 1. *Formulæ Medicamentorum Selectæ; or, Select Prescriptions of the most Eminent Physicians for various Diseases incident to the Human Body*. By EDWARD Fox, Apothecary in Ordinary to Her Royal Highness the Princess Amelia. London. 1777.

2. *Priestley's Experiments and Observations on Different Kinds of Air*. 1775.

3. *Cavallo's complete Works on Electricity*. 1777.

4. *Pareira's Materia Medica*. Vol. I. 1849.

5. *Snow on Anæsthetics*. 1858.

6. *The United States Dispensatory*. By WOOD and BACHE. Eleventh Edition.

7. *The Pharmaceutical Journal*. London. 1846—1871.

8. *Reports of the British Association for the Advancement of Science*. 1860—1871.

9. *The British Pharmacopœia*. 1867.

10. *Bond and Rockwell's Practical Treatise on the Medical and Surgical Uses of Electricity*. 1871.

11. *The Practitioner*. (Monthly serial.) Edited by ANSTIE. 1868—1871.

As to the book thus sent forth, it is a model of care and neatness and industry. It is thoughtfully compiled, splendidly printed and choicely bound. It contains four hundred and eighty-four pages and three times that number of prescriptions. It is divided in such manner that all the diseases of the community are classified, while the prescriptions for the cure of each class of disease are placed under their proper heads. The prescriptions are from the pens of none but the *élite* of physic; if the authors of them were dead or had retired from practice when the book was written, their names are added; if they were living, their names are omitted, "to avoid giving offence." The favorite recipes of princely Richard Mead, Sir James Millard, Huxham, Sir Clifton Winteringham, Sir E. Halse, Sir E. Barry, and of many others, are here in correct form. We think at once, as we look at this book, how easy it must have been to prescribe in 1777. We have a disease to treat; we know the name of the disease, and before us there is the remedy for the disease fresh from the most learned of hands. When we turn to our book and ask what the medical *élite* of the metropolis prescribed for the benefit of mankind, and for the imitation of their less fortunate brethren of the provinces, we note singularly and specially one great fact, that their formulas are all based on the most absolute of empirical principles. We notice further that the medicines prescribed are in nine cases out of ten simples of simples. There are, it is true, a few active medicines, to wit, opium, antimony, ammonia, elaterium, mercury, lead, sulphuric acid, jalap, rhubarb, lemon juice, and one or two other agents more or less active, but none of those really useful medicines, as we now consider them, take prominent place; prominence is rather given to carminatives and comparatively inert medicines—cardiac confection, red coral, chalk julep, stomachic tincture, musk, dragon's blood, Castile soap, oil of juniper, wormwood, James's powder, saffron, balsam of Tolu, syrup of marsh-mallow, cascarilla, ginger, gum ammoniacum, mace, camphor, and aqua pura; these clearly were the stock-in-trade, *par excellence*, of the apothecary to the Princess Amelia, and friend of the leading physicians of London, one hundred years ago.

For the different classes of diseases the prescriptions are strangely varied, no principle of action being anywhere indicated. For consumptions, spermaceti, made up with yelk of egg, with nitre, volatile aromatic spirit, and cinnamon water, forms one mixture intended for cough; viper broth, to be taken about midday, in four ounce doses, is another remedy; antimonial wine, with orange, cardamoms and white wine, is a third prescription, from the authority of Dr. James; and decoction of Peruvian bark with balsam of Tolu and vitriolic elixir forms a fourth combination. For dia-

betes, a mixture of oak bark and decoction of galls is placed first on the list; further on is a bolus of Peruvian bark with alum: two forms are offered for the "*aquæ benedict. comp.*;" and, again, a compound of "*terra japonica*," "*sang. macan*," and "*tinct. bich.*," to be made into a bolus. For diarrhœa there are pages of prescriptions, in which rhubarb, chalk, and musk, play the leading parts; we have also the decoction of intestines of a fowl, burnt rhubarb, red coral, *jusculi vervecini*, and many more strange simplicities. For dropsies we are favoured with some approach towards a principle of treatment, for both active diuretics and purgatives are prescribed: in one prescription the eighth of a grain of elaterium is ordered, in another jalap, in a third squills and juniper, and in a fourth squills and Venice turpentine. For elephantiasis mercurial ointment, with essence of lemon and oil of tartar, forms an unguent, and several forms of inert remedies are indicated for internal use; for erysipelas all the medicines are negative in character; for "*emissio involuntaris*" there is a good prescription, containing tincture of bark and tincture of cantharides; for epilepsy there is nothing better than valerian; for fevers mild salines, chamomile-tea, nitric ether, lemon juice and Peruvian bark, Hoffman's anodyne, and tartar emetic, are the favorite remedies, and are ordered with something of system; for intermittents Peruvian bark enters into every prescription; for fluor albus balsam of copaiba is denoted as the remedy; for gall-stones the medicines suggested are practically inert; for gout there are pages of set forms, the Duke of Portland's gout powder and Raleigh's confection having both important places; for whooping-cough tartar emetic is named; for itch a calomel purge and sulphur ointment are the two specifics; for lues venerea mercury is the leading preparation, but sarsaparilla and guaiacum are likewise put forward; for measles spirit of Mindererus is the curative agent; for rheumatism guaiacum, and for smallpox Peruvian bark.

On looking over the whole of this work of Fox it strikes us specially how much the practice of the most eminent of the medical fraternity of a hundred years ago resembles the practice of the community as apart from the practice of the medical fraternity of the present day. The drugs the old nurse or amateur doctor now calls for most frequently at the counter of the druggist were once the remedies prescribed by the learned in their best Latin.

Another point of interest is the almost absence in this book of forms of some of the present most potent remedies. Iron and opium are, comparatively, rare remedies; iodine, ordered at that time as burnt sponge, is rare; and alcohol, except as a vehicle for making a tincture, is scarcely mentioned. The terms used to

define and classify medicinal substances according to their action and properties are to our modern minds equally singular. Sarsaparilla, guaiacum, burnt sponge, Ethiops-mineral, antimony and taraxacum, are considered to be alteratives. Bark, rhubarb, balsam copaibæ, and calomel, are enumerated amongst the astringents; soap, gum ammoniacum, and millipedes, are put forward as diuretics; while the whole family of remedies now called tonics is left out of the vocabulary. Taking the complete list of the remedies formulated, prevailing inactivity is the most striking fact the list reveals: in many examples the remedies are so inert the modern homœopath might adopt them without the remotest injury either to his conscience or his faith; viper broth and stewed millipedes are signally at his service if he be prepared to use them discreetly. But there is one remedy which saved the fathers of a hundred years ago from all suspicion of a do-nothing treatment, and which explains why they were so often content with mild medicaments from the shop of the apothecary. They carried the remedy in their pockets, and it was not a child's remedy—it was the lancet. In apoplexy this was the remedy; in asthma, in fever, in hæmoptysis, in hæmorrhage, in hepatic flux, in measles, in pleurisy, in acute rheumatism, in smallpox, venesection held a first place in the modes of cure. With the lancet these ancient men brought down febrile heat, quieted laborious breathing, relieved congested lungs and heart, removed pressure from the brain, stopped hæmorrhages, subdued spasm, produced absorption of effused fluids, and reduced acute pain.

It will perchance appear to the readers of Mr. Fox's book of prescriptions that one hundred years ago the therapeutical art, leaving out always the question of venesection, was in a condition little removed from positive negation. The idea is the fact. The period was the centre of a transition in medical science and art, when men, in doubt, found it most convenient, both for their conscience and their courage, to take a negative position and hold to it. The Greek, the Roman, the Arabian, the Italian, the early English and German and French schools of medicine, had practised previously a bold if uncertain therapeutical art. The Greek and Roman masters had carried the administration of vegetable organic medicines to some perfection, and had learned the action of narcotics so well as to venture to give remedies that should make surgical operations free of pain; their Galenicals for centuries had held their way without disputation, and had lost no tithe of their credit till Bombast de Hohenheim, in the sixteenth century, thundered out, from Bale, his wild and mystical chemistry, making a new language, and bearing the rebuffs of his compeers with unmoved and, to them, exorbitant egotism of

new knowledge and new thought. The Arabian masters had retained the learning of the Greeks, and in the treatment of smallpox and some other maladies had advanced on their own account. The revived learning of Italy, Spain, Germany, France, England, had led to the employment, in the fourteenth, fifteenth, and sixteenth centuries, of mercury, antimony, and other metals, in opposition, largely, to the Galenical remedies, to many mechanical processes of cure springing out of the discovery of the circulation of the blood, such as transfusion and artificial respiration, and to the application—following sharply upon the discoveries of William Gilbert and Stephen Grey—of electricity as a further means of cure of disease. And yet one hundred years ago Fox could write his book, and the book could be the accepted mind of the time amongst its fellow-books of the same period, the acknowledged equal of all of its class, although a book so negative that honestly the two greatest remedies in it are venesection and Peruvian bark.

We have said the period was transitional. It is instructive to ask why that was the case. To answer this question we must look back a little into *personal* history.

The latter half of the seventeenth century had been marked in England by the presence of a strange man of the name of Gideon Harvey. This man, who had won the royal favour of Dutch William, and had obtained much popular favour, but who had meanwhile received every favour except popularity from his professional brethren, revived or introduced discussion on the system of doing nothing in disease. His work on the treatment of disease by the method of expectation, even though it might not have intended as much as it performed, and though it is a poor production, led the profession to trust to what is called the natural cure of disease, and introduced a conceit of inactivity of curative art unknown in all preceding epochs of medicine. A little later the illustrious Stahl astonished Europe by the boldness of his propositions, and as he adverted to Gideon Harvey, and in a certain sense supported him, the effect of Gideon's efforts was extended and maintained. Stahl, moreover, on his own account, turned over a new leaf in physic, entrusted to his followers the chemical theory of phlogiston, and, trying to trace out a chemical or chemico-physical explanation of the symptoms of disease, introduced a new era of medicine—new terms, new thoughts, new science. The theories of Stahl were, however, too advanced to be acted upon at once in matters of practice, and probably were understood by very few of medical practitioners. His phlogiston was used to signify "fire contained in bodies as a constituent principle," but it was assumed to differ from elementary fire, in that when it was

united to a body it communicated neither light nor heat, and produced no changes, either of solidity or fluidity. It was, moreover, a something that could not be perfectly separated; and while it was assumed, said the learned John Quincey, in his 'Lexicon of Physic,' that all inflammable substances contained phlogiston, it was assumed also that the non-inflammability of a substance afforded no proof of the absence of phlogiston, seeing that several metals that were not inflammable abounded with it. Stahl, consequently, presented to the practical minds of medical practitioners a theory that was profound and obscure; a theory advanced upon good experimental evidence, and yet not indicating any precise suggestion for the direct removal of conditions of disease. Briefly, the work of Stahl, while it weakened the old empiricism of medicine, did no more than weaken; it replaced nothing, it created nothing in treatment, except doubt, and with doubt fear.

Following upon the Stahlian doctrine another portion of personal history comes on the scene. At Halle there lived for many years, in the first part of the eighteenth century, a great scholar by the name of Michael Albertus. He was born in 1687, and became known as an author, in 1703, by an essay on hypochondriasis and hysteria. Thence onwards for a space of fifty-one years he continued to be the grand medical essayist of the world; his final dissertation, the last of over three hundred and twenty, being on the same subject as the first, 'De Morbis imaginariis Hypochondriacorum.' One dissertation every second month for fifty years is the average work this prolific scholar furnished to medicine, while he lectured in his own university on practice of medicine to students from all parts of the world. It is hardly possible at this time to estimate the influence exercised by this man on the course of medical science during the eighteenth century. Possessing less of originality and of genius than Stahl, and failing to found any acknowledged system of physic, if it be that he tried the experiment, he succeeded, by his industry and his almost whimsical erudition, to lead the multitude after him, as only a master amongst his fellows can lead. His influence permeated in every direction, tinging general not less than medical literature. The mere titles of his essays became texts of other men's works and conceits. Unhappily or happily, it was in therapeutics that Albertus made himself most felt. In speculative doctrines on modes of cure he indulged almost riotously, and we retain to this day the bright train of his fancies. He gave the followers of Gideon Harvey a notice, and helped to sustain the principle of cure "per expectationem." He was eloquent on the subject of natural cure, and of the influence of the mind over the body

in process of cure; and he believed in the energy of nature, "*energia naturæ*," as a superior something over matter. Thus he was led away from the idea of cure by art, and threw the sick back upon nature as upon the only true physician. He did more than these things, he set up the grand schism of medicine which through Hahnemann was to divide physic into jarring classes. His work '*De Curatione per Similia*,' published in 1734, was the keynote of what has since been called "*homœopathy*;" his work '*De Curatione per Contraria*' was the keynote of what has been called—although the system, by the way, never existed except in excited and angry minds of the *per similia* sect—"allopathy."

When to these facts relating to Albertus we add that the leading teacher of practical, as distinct from speculative, chemistry in the eighteenth century, and at the same time the leading practitioner of medicine, the illustrious Boerhaave, had rather unsettled existing curative art than supported it, we are relieved of all surprise that the apothecary to the Princess Amelia and his contemporaries should have been in a state of doubtful inactivity in the practice of therapeutics, and that from the art of their day should have sprung an organization which symbolised their negations.

The date of one hundred years ago marks the era weakest of all eras in medicine. The world then was certainly not killed by its doctors, but assuredly it was not cured by them. And yet the time, at once negative and critical, was a great period in the history of practical physic.

This is no paradox.

While Stahl, Boerhaave, Albertus, and their compeers, had been laying low the empirical systems that had for centuries held sway, they were building new systems out of which new power was to flow and new practice take foundation.

Let us glance at what was looming a hundred years ago—let us glance at the works the lethargic doctors of the Foxian school were too young to know, too old to learn.

At this particular time the thought was breaking forth that the atmospheric air is not a simple elementary substance, indestructible and unalterable. Priestley was just at this time mounting his twelve-inch burning lens, that he might bring the sun to bear with effect on "*mercurius calcinatus*" and other substances held in vessels containing mercury, and might extract from these substances new gases, or rather gases new in his time to the sense of the chemist and physician. He was thus led to produce dephlogisticated air; the air that would let a candle glow in it with a "*remarkably vigorous flame*," and would let a piece of red-hot wood sparkle in it "*exactly like paper*

dipped in a solution of nitre;" the air we are all now familiar with as oxygen gas. Connected with these and after experiments of the illustrious Priestley, the practice of collecting gases became general, and pneumatic chemistry became advanced into a definite scientific procedure. A little later, in the hands of Lavoisier, the discovery of oxidation and the new views respecting combustion, which marked the advent of what has been called the antiphlogistic theory, had their birth. The discoveries of certain properties of particular gases once started, the effects of these gases on animal life came under notice. Priestley again took the lead in this research, and the inhalation of gases passed into a line of distinct experiment. First there came from Priestley the discovery of the influence of vital air or oxygen; and later, Beddoes, working in the same field, opened up the method of treating diseases by the simple process of making the sick inhale various kinds of factitious airs. A step further, and Sir Humphrey Davy, learning much from his master, Beddoes, inhaled nitrous oxide gas, and, discovering the insensibility to pain induced by it, suggested that it might be applied for the annihilation of pain during the performance of surgical operations. From these investigations we trace the introduction of a new era in physical therapeutics.

While the discoveries we have thus named were in progress another agency, in time to become an important therapeutic, was in course of advancement.

In 1777, the year in which Fox's book of prescriptions appeared, Mr. Cavallo published a work on electricity, in which he pronounced strongly in favour of electrical action as a cure for many diseases. Mr. Cavallo—who, by the way, was the first philosopher to suggest, in an experimental way, the application of electricity for the conveyance of intelligence from points of the earth at a distance from each other—was a man of vast attainments, and his reports of cases of cures by means of electricity created, for the first time, a profound interest in the medical world. The after discovery of galvanism, in 1787, supplied the means of applying both static and current electricity in the treatment of disease, and these means, carried into our time, have become, especially of late years, of a value in medical practice that remains still to be brought to completion and perfection.

Priestley, Cavallo, Lavoisier, Davy, and their allies, pursuing their respective paths, were accompanied by other men, opening up again new lines of research. Boerhaave, to whom we have referred, now comes into a new light as the practical originator of organic chemical therapeutics. By his attempted analysis of animal substances, by his separation of urea from urine, and by his analysis

of vegetable products, this immortal master opened up systematically the study of that vegetable organic chemistry to which modern medicine is so much indebted. The separation of the active principles of plants marks thus a third distinct advance in the progress of therapeutical research, handing down to us the suggestion that the medicinal properties of drugs lie, not necessarily in the crude mass, but in individual parts that may be isolated, proved, and applied at last with a precision compatible only with exact knowledge of quality and composition. It is very difficult to convey to the modern reader the services rendered to modern therapeutics by Boerhaave. In our time no man has approached him in industry and original labour. In honest truth, he puts to shame, not individual modern men alone, but actual societies of modern men bound together for common labour. He dealt with medicinal substances as true chemical substances, he sought in all he did to reach essences, he framed the outline of physiological research in respect to action of remedies, and he supplied, from his laboratory, to the world of science new series of active agents, some of which to this day retain their name, their place, and their power. Further, he invented for us processes, methods, and phrases which are basic. "A chemical solution;" "a chemical precipitation;" "a chemical coagulation;" "a chemical effervescence;" "a doctrine of alcalies and acids." These, and numerous other familiar thoughts float down to us from this marvellous labourer and become our own. Following Boerhaave, manipulators became more competent for the manufacture of refined organic compounds, such as rectified ether, which substance passed into medical use, on a large scale, upon the publication, by a Surgeon in Liverpool, Mr. Turner, of a paper entitled, 'An Account of the Extraordinary Medicinal Fluid called *Æther*.'

One more change relative to the science of therapeutics before the close of last century, and we have, we think, summarised that which relates to the influence of experimental research of the epoch on curative art. Priestley, so soon as he commenced to separate and to try the action of different gases, observed that some gases had a preservative action—that is to say, they prevented organic decomposition. He noticed this fact first, strikingly, in his researches on carbonic acid gas, and, with the singularly happy turn of mind that led him always to apply the new knowledge he had acquired to some useful purpose and end, he immediately put forward the plan of preserving dead animal structures in an atmosphere of fixed air—carbonic acid. Soon afterwards he proceeded to recommend the administration of carbonic acid in medicine, first for the treatment of open fetid sores, and next for the rectification of fetid conditions of

the stomach and intestines. He showed, in illustration of this latter point, that carbonic acid might be taken into the stomach without risk and with advantage. He directed that water should be charged with carbonic acid, and he invented the method of producing carbonic acid by combining acid and alkaline powders, at the same moment, with water. Thus the art of making soda water and of prescribing saline effervescing drinks comes from Priestley; and what is of still more interest, the plan of treating diseases by antiseptics springs, in a true scientific way, directly from his original and active mind.

In respect to the inhalation of gases, the applications of electricity, the separations of active medicinal from crude medicinal substances, the discovery of the antiseptic properties of certain gaseous products, we owe thanks to the workers of the last half of the last century. That the works of the workers did not at once rapidly develop into art is matter for no surprise. In the preceding time it had been held as an absolute principle that atmospheric air was a simple elementary body; and that it was one of the representative bodies of an elementary kind, was an idea so fixed in the mind that years were required to dispel the illusion, an illusion on the dispersion of which the progress of medicine in no small degree depended. The doctors had to unlearn and to learn—unlearn the first lesson of their school chemistry on air, and learn that what they considered as one air pure and simple consisted of two parts, a part which would not burn but would support burning, and a part which would neither burn nor let burn. They had, further, to learn the much more difficult lesson, when it was originally propounded, that a portion of atmospheric air may, actually, form a portion of fluid and solid things. Hitherto they had surmised that air could only permeate a substance mechanically, as water might permeate a sponge; now they had to accept that it might be a component of a number of things, of a rock, of a vegetable, of a man himself; that it might give weight, substance, form, even solidity. This was, indeed, a revolution of knowledge, and it had to be followed by the further revolution that water, another element as it had been implicitly believed, was also, like air, a double of chemical elements; that it had in it one element identical with the element in the air that would support burning, and another element (surely at first sight phlogiston itself) that would burn fiercely—hydrogen.

Thus closed the eighteenth century, in doubt, in wonder, and hardly yet in hope for the physician.

The beginning of the present century, preceded by the labours to which we have drawn attention, was the first era of true physical and experimental research in therapeutics. It was at the

same time an era, strongly tinctured with three or four attempted systems or universalities of cure. By this time Hahnemann had enlarged on the work 'De Curatione per Similia' of Michael Albertus, and had planted the flag of homœopathy. Brown had put forward the Brunonian theory of disease, had introduced the terms *sthenic* and *asthenic*, and had brought into therapeutics the special hypothesis of stimulants, by which view all medicinal substances, however different they might seem to be in action, were considered as stimuli. From the employment of cold affusion by Dr. Currie, of Liverpool, the system since called hydropathy, had taken root. Lastly, the remarkable work of Dr. Hamilton on the utility and administration of purgative medicines had become one of the standard books of physic, and had laid the foundation of what has been designated, with undue contempt, the pill and draught method of treating all diseases.

During the last sixty years these systems, aiming at universal cure, have shared different fates. The homœopathic dogma gaining adherents rather by the opposition that has been fiercely levelled at it than by any sign or quality of truth belonging to it as a sound hypothesis, has been cast out of the pale of scientific medicine altogether, in anger noisy enough to make the very bones of Albertus shake in their shell, that they should ever have been even the passive agents in the origin of so bitter a strife. The Brunonian speculation has held place by virtue only of one or two terms that were originally employed to define its principles. The method of treatment by cold affusion having passed through a schismatic phase under the head of "hydropathy," has, in these later times, been revived with some approach to scientific accuracy. The purgative treatment has, with some modifications, held its own.

One means of cure we have reserved for a final word before we turn the current of our discourse. We have indicated that Fox and his contemporaries had one great remedy which they used freely, viz. the little preparation of steel called the lancet. This remedy and its accessories, in the shape of the cupping-glass and leech, held almost undisputed sway until the years 1830 to 1845. During that period the practice of taking blood came into disrepute, and so vehement was the reaction against it, it has for a long time been, we had almost said, utterly ignored. It is very difficult to explain the reason of this change of opinion, although various reasons have been assigned for it. Some have assumed that the type or character of disease has undergone change, and that so-called sthenic disorders demanding the use of the lancet have been less common than they were; others have assumed that the practice of homœopathy has exerted a controlling influence; and again it has

been surmised that the remedy itself was an entire error, and that the superior wisdom of modern physicians has led them to detect the ancient fallacy, and to avoid falling into it. For our part we incline to none of these views; we think that bloodletting went out so suddenly because the rules that were given for its use, originally empirical, sweeping, and dogmatic in character, became impossible at a time when dogmas of every kind, and hard and fast lines of all dimensions were being broken without mercy, and when no substitution of new rule was ready for instant service. Such is our opinion on this curious historical subject, but as it is opinion merely, let it pass, and let us return to this matter of fact, that whatever may have been the reason for the abandonment of the oldest remedy in the art of physic, the abandonment was cowardly, loose, false, and opposed alike to the credit of medicine and to the welfare of mankind. The abandonment led for years to the desertion of treatment in many cases of disease, and thereby to the consignment to inevitable death of cases that ought not to have been deserted, and that would not have been consigned to death without attempt at cure had the professors of healing been conservative enough to have saved from temporary destruction those parts of the practice of bleeding that were precious, and bold enough to have held fast, before the ignorant world, that which was good. Happily, the error of the entire abandonment has, at length, been recognised and to some extent rectified. It is seen that bloodletting, itself a mechanical act, is a remedy of first value for the relief of symptoms dependent on mechanical pressure of blood upon the primary organs concerned in living action.

And now before our brief review of the century of therapeutical science is concluded, let us look for a moment at the latest phases of progress and the paths towards which they point.

We recognise, firstly, then, that the curers of the present day, in the enjoyment of realising much that their predecessors of the earlier part of the century were preparing for them, are relieved of many of the doubts belonging to the transitional state of belief that, previously, so long prevailed. It is not to be denied that still there linger amongst us a few elegant doubters who prescribe carminatives for dyspepsias, alcoholics for hypochondrias, and placebos for acute diseases, and for whom the medicine book of Mr. Fox, with its receipts for the "Duke of Portland's gout powder," for "Tinctura Stomachica," commonly called the "Bishop," and for the "Tinctura Stomachica Cephalica," commonly called "Burgundy," would be a windfall of the first quality. It may be that a few still linger of the heroic school,

eminently practical men, who have a bold remedy for every disease, "learned Sir by experience and always sure;" but these of both classes are fast passing away. As for the majority, they are beginning to regain faith in the actual principle of cure, and are appreciating the absolute necessity of physical research in relation to all physical methods of cure. We readily confess that many marks of childish weakness are still daily exhibited; and the readiness with which a new remedy is seized upon, prescribed, adjudged upon, lauded, abused, and without reason often thrown aside for a newer remedy that has not been tried, is, we admit, deplorable. At the same time the weakness exhibited is not the weakness of decline; it is the weakness of a new life, of a learning life, in which the tendency is towards a restless capricious mode of acquiring knowledge, a mode that will become steady and sure as it is learned, proved, accredited, advanced. Respecting the mode of advance itself, it branches out into so many physical directions, it is hard to follow it minutely. We notice, therefore, general principles or institutions only and these in outline.

The properties of agents used for the cure of disease are in this day being carefully studied in respect to physical action. We ask of a solid or fluid substance, does it belong to the colloidal or to the crystalloidal group of substances? Will it or will it not diffuse in the animal organism, and if it will diffuse, what is the rate of its diffusion? We estimate in some degree by these inquiries rate or activity of action, and the general or systemic as distinct from the local action of medicines.

We inquire as to the action of agents from their chemical alliances. We ask, will a chemical substance produce in its action effects similar to those produced by another chemical substance belonging to the same series? Will phosphorus, for example, act curatively in the same manner as arsenic? This method of research, introduced by one of our accomplished workers, Dr. Broadbent, is full of promise. It will of necessity be modified as our chemical knowledge of elementary bodies is remodelled, but it will probably follow chemical discovery very faithfully, and be a steady progress in therapeutical science.

We inquire again as to the relative action of chemical agents in connection with their atomic weights, and their degrees of solubility. In the study of gases that are to be inhaled for the production of anæsthesia, these points are of primary import; and since the time when Snow conducted his admirable researches, they have been carefully considered and with exceeding advantage. They are points also which have been thought deserving of attention in investigations bearing on the action of

the different alcohols, of the organic iodides, bromides, nitrites, and other organic compounds.

The physiological properties of various active medicines are now investigated, on the basis of their chemical composition, and of the changes introduced by substitution. Drs. Crum Brown and Fraser have here laboured with consummate industry in relation to substitution compounds of strychnia and nicotina. Observations of a similar kind have conducted us to the modifications of action which occur from modifications of construction upon various organic bases, as upon the radicals methyl, ethyl, butyl, and amyl. From these inquiries it has been found possible to predict, in a certain degree, the action of a substance from its chemical composition and physical quality.

Quite recently another research entirely new has been followed up on the question of the decompositions of chemical substances within the body. It was ascertained a few years ago from the experiment of inducing artificial cataract in frogs, by the introduction of crystalloids (salines) into the bodies of these animals, that all salines do not produce this phenomenon; that chloride of potassium, for instance, produces the cataractous state, while iodide of potassium fails to do so. Hence it was suggested that the iodide salt is decomposed within the body, and that new products, inert, in so far as action upon the lens is concerned, are evolved in the organism. The discovery of the physiological action of chloral hydrate, by the distinguished Liebrich, has brought this subject of decomposition of chemical agents within the body into more conspicuous position, and has advanced a line of experimental labour that will in time extend itself to the whole course of therapeutical learning.

The physiological antagonism of different medicines has afforded a theme for great and laborious inquiry in modern days. The antagonisms of belladonna to morphia, and of nitrite of amyl to strychnia, have been studied with interest, and although the problems that have been sought for solution have not been made absolutely demonstrative, and, indeed, in some cases have been contradictory, practical usefulness has followed the attempt.

An observation first made by Dr. Weir Mitchell of Philadelphia, a physician whose keenness of intellect is equalled only by his endowment of fidelity in delineating natural facts and commenting upon them, has led to a careful investigation of the action of medicinal agents on different classes of animals. Dr. Mitchell noticed that pigeons were able to take, with impunity, doses of opium and morphia that would prove poisonous to man. His experiments, repeated in this country, were decisively confirmed, and upon them many new investi-

gations have been instituted. Dr. Elliott, of Hull, has shown that doses of nicotine poisonous to man may be taken by goats without any danger; and Dr. Wood, of the University of Pennsylvania, has proved that belladonna, stramonium, and hyoscyamus are as innocent to birds as is morphia. These curious and all-important truths, when fully matured and reasoned upon correctly, will explain much that has, so far, been obscure, relative to the difference of operation of the same remedies on different persons and on the same person at different periods of life and under different conditions of existence.

The action of various substances, used as remedies, on the temperature of the body has been matter of further and minute study. It has been shown that chloroform, hydrate of chloral, and quinine, produce, when they exert any decisive effect, a reduction of the animal temperature, an effect as far as has as yet been discovered common to medicinal agents. We do not assume in stating this that there is no medicinal substance capable of causing an elevation of temperature; we say simply that no such substance has yet been found. In truth, there lies before us a splendid mine for exploration. We may discover substances that produce temporary surface heat by causing paralysis of vessel, exposure of blood over an extreme surface and quick radiation, which substances would all be relaxants, and, in the end, cooling agents. We may find other substances that produce definite increment of heat with constriction of vessel, and these, if found, would all be astringents and styptics.

The earlier observations on absorption by the blood, and the rapidity of transmission of certain highly soluble substances led, some forty years since, to the practical revival of the humoral pathology. It was assumed that all actions of medicines were by the blood, and thereupon, by speculation without experiment, the blood ceased to be considered a river conveying to the circumferences of the body that which it simply carried, and was thought to be also a laboratory or manufactory. Thus the nervous system sank into a secondary position, and the doctrine of solidism passed into nothingness. Here was a startling innovation which at this moment is slowly being rectified, not by the equal error of turning the balance straight over to solidism, which in the concrete is absurd, but by setting the balance level and proving that while the nervous centres are reachable and are reached by different agencies travelling by and through the blood, they are also reachable and reached by agencies directly affecting them, that is to say, independently of the blood altogether. Further, it may be suspected that the blood itself can be physically changed by and through the nervous

organism. These researches on direct and indirect action of medicines upon nervous matter have led to precision of observation on the respective influences of various substances on distinct portions of the nervous organism and on the two nervous systems, the organic and volitional. We know, for example, of certain agents which appear to act purely on the system of nerves that governs the vascular supply. We know of some agents which paralyse or exalt the peripheral nervous surface, of others which depress or exalt the centres of nervous matter, and in one or two instances we are able to differentiate the centres themselves that are particularly influenced. More strangely still, we are learning, in respect to the influence of certain chemical substances, that there is a resemblance in their action to the action of those obscure and, we had well nigh said, metaphysical influences that induce the phenomena known as emotional. The nitrites of the organic radicals are among the refined agencies which excite what would be called mental changes, were the results they produce not clearly traceable to a purely physical cause.

In conclusion, for we are at the border of the space accorded to us, the late researches that have been conducted on the effects of alcohol, nitrite of amyl, and some other substances, upon the rapidity of the motion of the heart, afford us a knowledge as immediately practical in the treatment of disease as it is theoretically useful for the final study of the classification of medicines according to their simple and acknowledged physiological intention.

The work of the past hundred years of therapeutical science and art has not been work misspent. The art has been at times cowardly, at times fitful, at times perverse, at times angry, at times fanciful and dilletante, at times firm and serious, and at times apparently hopeless—an art without a science. Lately the art has submitted to follow the science; but with the most prosperous gale of science to fill its fullest sail, it must be content to proceed slowly, obstacle upon obstacle rising in its way. Our pharmacopœias still show how beggarly is the approved standard of healing physic. Our still existing great schisms, and the fanatic faith that sustains them, still indicate how little the public is prepared to follow the real leaders in the process of cure, and how profoundly it relies on dogma rather than on knowledge purged of dogmatic assertion.

We who would advance must, therefore, not only advance boldly under difficulty, but must conquer our own doubts, by perfecting still our knowledge, lest our doubts be traitors to our cause.

II.—The Osteology of Mammalia.¹

THE tediousness of osteological works has been a fertile source of accusation against anatomists since the time of Bélon. The old and quaint French author, in his amusing treatise, ‘Sur les Oyseaux,’ said—

“Nous scavons cōbien plusieurs qui voudroyent avoir incontinent comprins toutes choses sans y travailler, aiment la briefveté des escripts; et combien elle est odieuse à aucuns, qui pour le désir qu’ils ont de les comprēdre ne se sentent molestez de les lire.”

To revolutionise entirely the classification of Mammalia, to alter it from the position at which it stood at the time of the publication three years ago of Professor Owen’s ‘Comparative Anatomy of Vertebrates,’ is the task which Professor Flower has set himself. That a work, every part of which is of the most controversial nature, should yet not contain a single sentence of polemical bitterness, is a fact which redounds alike to the discretion and learning of Professor Flower. Free from the servile adherence to the dogmas of his great predecessor, Professor Owen, exhibited by many of that great anatomist’s ardent admirers, he is also free from a prevalent fault of seeking to erect a reputation on the fame of others. Professor Flower’s lectures undoubtedly constitute one of the most valuable contributions to the science of comparative anatomy that has appeared for many years. The arrangement of the class Mammalia differs in most respects from that of previous authors, including Cuvier, Illiger, Home and Owen, although it somewhat resembles that of Mr. Waterhouse. Three great groups are formed, viz. *Ornithodelphia*, *Didelphia* and *Monodelphia*. Of the two former sub-classes we shall say nothing. The lowest order of *Monodelphia*, that of the *Edentata*, is detached from the rest by a long interval. The *Pecora* and *Tragulina* are placed lowest among the *Ungulata*. The *Tragulina*, represented by the small group of *chevrotains*, allied to the musk-deer, were by Milne-Edwards invariably separated from their nearest of kin, and in the present work Professor Flower has failed to indicate the position of the musk-deer, but we presume that they would find their right place among the *Pecora*. The *Suina* and the *Tylopoda* then follow, and above them are ranged the *Perissodactyla*. Our own impression with regard to this arrangement certainly would be that the *Perisso-*

¹ *An Introduction to the Osteology of the Mammalia: being the substance of a Course of Lectures delivered at the College of Surgeons of England in 1870.* By WILLIAM HENRY FLOWER, F.R.S., F.R.C.S., Hunterian Professor of Comparative Anatomy and Physiology, and Conservator of the Museum of the College. With numerous illustrations. London, 1870.

dactyla and *Artiodactyla* should rather be considered as separate and co-ordinate orders originating under like circumstances at the commencement¹ of the Eocene epoch of geological time, and that neither *Perissodactyl* nor *Artiodactyl* could be said to be superior to the other. However, we have not yet got to the end of the apparently inextricable maze in which we are placed. A gap on one side separates *Perissodactyla* from *Proboscidea*, and a still wider gap on the other side separates the former from the *Sirenia*. Is this right? Are not the extinct *Toxodonts* an annectant family between the *Proboscidea* and the *Sirenia*, and between both these and the *Rodents*? We fail to see the *Toxodonts* anywhere in the present diagram, and we should suggest that Professor Flower in his next diagram should place *Sirenia* and *Proboscidea* in closer juxtaposition. The affinities of the *Sirenia* with the *Cetacea* are purely adaptive, perhaps merely mimetic, and we cannot conceive that so accurate a comparative anatomist as Professor Flower should violate the natural affinities of *Mammalia* so far—for the mere sake of getting the seals into line with the whales—as to place the manatis and the elephants on different sides of the same diagram. If further evidence were lacking, the affinities of the *Dinotheres* ought to be perfectly conclusive. Higher up we have the *Rodentia* and the *Hyracoidea* forming separate ordinal types, while the *Insectivora*, *Carnivora* and *Cetacea* range across the diagram. Professor Flower has so arranged his diagram as to bring the *Lissencephala* of Professor Owen, comprised under the orders *Edentata*, *Rodentia*, *Insectivora* and *Cheiroptera*, on the same side as the aberrant (from the mammalian type) *Didelphia* and *Monodelphia*. The *Cheiroptera* and the *Primates* form the top of the diagram. The latter are divided into three great groups, *i.e.* the *Lemurina*, the *Simiina*, and the *Hominina*. The latter are made in the present system to be less aberrant from the common mammalian type than the *Cetacea*. Such is Professor Flower's system, which, if not in accordance with those of previous authors, assuredly possesses the charm of novelty.

Professor Flower's nomenclature is often facile and convenient; thus, he wisely substitutes the term "thoracic" for "dorsal" vertebra when speaking of the vertebræ in *Mammalia*. The chief assertion relative to vertebræ is comprised in the following sentence:

"At present it does not appear that there is that uniformity in the plan of construction of all vertebræ which has often been supposed, and definitions of the different parts applicable in every case have not yet been arrived at, and it may even be doubted whether this will ever be possible."

¹ The affinities of the *Stereognathus* of the Oolite are confessedly doubtful.

This assertion, of course, is destructive of the doctrine of serial homology or homotype of all the vertebræ in the spinal column of Mammalia.

Now, Professor Flower, by restricting the scope of the present volume to the Mammalia, has deprived critics of the opportunity of reference to the more simple vertebrates, in which the vertebral column, admittedly constructed on the same type as that of Mammalia, exhibits the undoubted repetition of similar and answerable segments. The precept of Goethe—

“Willst du dich am Ganzen erquicken
So musst du das Ganze im Kleinsten erblicken”—

emphatically applies to the recognition of the serial homology of vertebræ, which is the more easily comprehensible in such low forms, *e.g.* as *Amphioxus*, than in the higher and more specialised mammalian forms. Owen well defined the distinction between homology and homotypy, and we make no excuse for reproducing his canonical definitions.

“*Analogue*, a part or organ in one animal which has the same functions in another part or organ of a different animal.

“*Homologue*, the same organ in different animals in every variety of form and function.

“The little *Draco volans* affords a good illustration of both relations. Its fore limbs, being composed of essentially the same parts as the wings of a bird, are homologous with them. Homologous parts are always, indeed, analogous parts in one sense, inasmuch as, being repetitions of the same parts of the body, they bear in that respect the same relations to different animals. But homologous parts may be, and often are, also analogous parts in a fuller sense, viz. as performing the same functions; thus, the fin or pectoral limb of a porpoise is homologous with that of a fish, inasmuch as it is composed of the same or answerable parts, and they are the analogues of each other, inasmuch as they have the same relation of subserviency to swimming. So likewise the pectoral fin of the Flying Fish is analogous to the wing of a bird, but, unlike the wing of the Dragon, it is also homologous with it. . . .

“The corresponding parts in different animals being thus made namesakes, are called technically *homologues*. The term is used by logicians as synonymous with ‘homonyms,’ and by geometricians as signifying the sides of similar figures which are opposite to equal or corresponding angles. . . . It will, of course, be obvious that the *humerus* is not ‘the same bone’ as the femur of the same individual in the same sense in which the humerus of one individual or species is said to be ‘the same bone’ as the humerus of another individual or species. In the instance of serial homology above cited the femur, though repeating in its segment, is not its namesake—not properly, therefore, its homologue. I propose, therefore, to call the

bones so related serially in the same skeleton 'homotypes,' and to restrict the term 'homologue' to the corresponding bones in different species, which bones bear, or ought to bear, the same names."

It is true that in some bones of the skeleton many authors have confused the laws of general and of serial homology. Thus, Dr. Camille Bertrand, in his work entitled '*Conformation osseuse de la Tête chez l'homme et chez les vertébrés*,' reproduced Professor Owen's system of homology, but differed from that author in various minor respects, *e.g.* in the non-recognition that the scapular arch is the hæmal arch of the occipital segment. Failing to perceive this affinity, Dr. Bertrand recognised the hæmal arch in the thyrohyal bones (= hypobranchial of fishes), whilst the other hyoid bones (*e.g.* glossohyal, basihyal, stylohyal, ceratohyal, epihyal, urohyal) are component parts of the parietal vertebra. Our space necessarily precludes us from entering into this delicate and interesting subject of Transcendental Anatomy; we will merely remark that we do not think that Dr. Bertrand demonstrated the serial homology of the thyrohyals as occipital hæmapophyses.

Professor Flower's classification of the peripheral parts of vertebra into autogenous and exogenous is a convenient modification of that adopted by Professor Owen. He, however, points out distinctly that parts of a vertebra which are serially homologous with each other are often either autogenous or exogenous. We thus arrive at an element of confusion of the most perplexing nature. The confusion which often may exist between the metapophysis, the zygapophysis and the anapophysis, may be rendered inextricable by the acceptance of this conclusion of Professor Flower. Indeed, so far is the whole terminology of the science revolutionised by this new proposition that we confess that we do not quite know where we are, nor how far the science of philosophical anatomy may be turned upside down by it. One element, however, in the typical vertebra appears, according to Professor Flower, to be certain. This is the hypapophysis of Owen, whose function as a downwardly depending spine, extending the whole length of the articulated centre of the cervical vertebræ in some Ungulata, is well described by the author. Apparently one fact is, however, elided by Professor Flower, *i.e.* that both the hypapophysis and the epapophysis often form two separate rings, the one above and the other below the centrum, and internal to the neural and hæmal arches respectively. This fact may, perhaps, if duly considered, throw much light on the serial homologies of the branchial cartilages and arches, and may, perhaps, explain the (at present) rather puzzling collocation of Meckel's cartilage with the mandible in the process of development. If some of

the perplexing structures which occasionally divaricate on either side of or below the centrum are carefully examined, we doubt not that they may prove to be not necessarily hæmapophyses or pleuropophyses, but merely exogenous hypapophyses. Professor Flower certainly lays greater stress on the autogenous nature of the hypapophysis than at present we are prepared to admit, although we are far from denying that the hypapophysis may in a great number of instances be recognised as truly autogenous, a fact which some of the earlier authors denied. So far there is much probability in favour of Professor Flower's theory.

Having sketched the general characters of the vertebræ of Mammalia, the author devotes three long chapters to the consideration of the cervical, the thoracic and lumbar, and the sacral and caudal vertebræ. In the chapter relating to the cervical vertebræ a reason is tacitly given for the fact stated by Duvernoy, that in some Hottentots the neural spines of the cervical vertebræ are not bifurcated. Some exceedingly good diagrams are given of the anterior surface of the vertebræ in the dolphin and in the sperm whale; four sketches being given of the fifth, seventh and eighth thoracic and first lumbar vertebræ in the former animal, and the eighth, ninth, and tenth thoracic and fifth lumbar in the whale.

Professor Flower's researches on the skull differ entirely from those of most previous writers. Selecting the skull, *e.g.* of a dog, he gives a sectional view, in which the numeral system which Professor Owen has adopted with such success in the numbering of bones is set aside, and a scheme of irregular letters substituted. It is incidentally pointed out that when the zoologist wishes to throw into the—

“Strongest relief the distinctive characters of different species, he selects for comparison fully adult examples; when the anatomist wishes to trace their community of structure and their resemblances, younger specimens are better adapted for his purpose.”

He adverts to the scheme of Rathké and Müller respecting the cranio-facial axis, comprising the basi-occipital, basi-sphenoid, and presphenoid bones, and remarks that—

“So far the cranio-facial axis consists of bones placed in a continuous line, more or less depressed, and broad from side to side, and forming the floor of the cranial cavity; but the continuation of the axis forward is of a different character. The anterior end of the presphenoid narrows considerably, and the segment in front of it, in very young skulls, is a much compressed vertical plate or cartilage of very considerable size, both from before backwards and from above downwards. Ossification of this cartilage commences in the

posterior end and upper part, and spreads forwards and downwards, but it never, or very rarely, reaches its anterior extremity, and in the animal now described a narrow inferior margin remains permanently cartilaginous. The ossified portion of this cartilage constituted the *lamina perpendicularis* of the ethmoid bone, the anterior unossified portion, the septal cartilage of the nose, which is the anterior termination of the cranio-facial axis. The term *mesethmoid* may be applied to the whole of this element of the skull, whether ossified or not."

This term *mesethmoid* is not used in many other contemporary authors, but it is doubtless of great value.

The description of the skull of the dog is certainly lucid, but we think that, both for students and for the more profound osteologist, Professor Flower would have done better if, when describing a bone of the skull, he had clearly defined its connections with those immediately before and behind it in the vertebral series. For a learner, *e.g.* to be told that the basi-sphenoid part of the sphenoid and pre-sphenoid may mechanically form an integral part of the bone behind them, the basi-occipital is a fact, *quantum valeat*, duly recounted. But is it one of any value, or is it worth the knowing by an osteological student? The true fact, which should be taught as early as possible, is that the pre-sphenoid, the basi-sphenoid, and the basi-occipital, form alike component parts of one uniform series of central bones, similar to those *centra* of vertebræ which extend along the whole of the spinal column.

Professor Flower's account of the human cranium is thoroughly comprehensive, and we are pleased to see that he commences his analysis of the bones by considering the skull vertically bisected. This plan is always the most convenient to adopt. In his diagram on page 127 the same views of the homology of the stylohyal are adopted as in the majority of the previous writers.

The communication, however, which the author made to the British Association in September, 1870, gave a new interpretation of the morphology of the hyoid arch, one which is of the deepest interest, both to those who agree with and those who differ from him. If we collate his views with those of Camille Bertrand above mentioned, the divergence between the two is most significant, and may, perhaps, lead future observers to a more accurate appreciation of the general homologies of the hyoid arch.

The distinctions between the skull of apes and man are pointed out at great length. The following passage is very interesting:

"The occipital region of the skull, with the foramen magnum

behind and the cribriform plate of the ethmoid in front, are in the same general horizontal line with the basi-cranial axis as in man, and not perpendicular to them as in the dog. It is remarkable that the deviations from this general rule, especially as regards the plane of the occipital surface, are not in relations to the general position of the animals in a descending series from man to the lowest monkeys, for the occipital surface is nearly vertical in the anthropoid gibbons (*Hylobates*), especially *H. syndactylus* (the siamang), and completely so in the American howling monkeys (*Myctes*), where the cerebral fossa does not project in the least degree behind the cerebellar fossa; while in the baboons (*Cynocephalus*), among the old-world monkeys, and still more in some of the smaller and lower forms of American monkeys (as *Callithrix*) the posterior development of the cerebral fossa is so great as to throw the supra-occipital bone considerably more into the posteriorly prolonged base of the skull than even in man."

With regard to the *Carnivora*, Professor Flower refers at great length to his paper in the 'Proceedings of the Zoological Society,' 1869, on the value of the characters of the base of the cranium in the classification of the order, sections of the tympanic bullæ of the tiger and bear being taken as examples. A remarkable fact is adverted to respecting a new genus of Rodentia:

"In a very remarkable East African genus, *Tophiomys*, a broad bony lamella extends from the upper part of the parietal outwards and downwards, to join a similar ascending plate from the malar, and so forming an arched covering to the temporal fossa, an arrangement unknown in any other mammal, but recalling that met with in the tortoises. The whole of the superior surface of the cranial bones of this animal is covered with miliary granulations disposed with perfect regularity and symmetry,"

We believe that this combination of Glyptodont and Archeosaurian characters is at present unique. With reference to the skull of the pig, Professor Flower showed that "at the anterior extremity of the mesethmoid a peculiar ossicle (prenasal) is developed, which strengthens the cartilaginous snout." This fact is most significant, as it indicates to those who follow Professor Macdonald, of St. Andrew's, in his speculations of 'The Physical Characters of the Angels,' that the pig is the next animal who is to develop the forthcoming vertebra in front of the nasal. Certainly, according to the learned Scotch professor, man is a little lower than the angels, but pigs are undoubtedly next to them.

The following facts respecting the hyoid arch of the *Hyracoidea* are, we believe, perfectly new and very striking:

"The hyoid apparatus of the Hyrax is unlike that of any other known mammal. The basihyal is oval, transversely extended and

flat, with a small median eminence on its anterior border, and an emarginate posterior border, only ossified in centre, and prolonged laterally without any definite segmentation into broad, flattened, slightly curved, cartilaginous thyrohyals. Articulated to the anterior and external angles of the basihyal are two large, triangular, flattened bones (ceratohyals), each with a long process projecting forwards, and meeting in the middle line, so as to enclose (with the anterior margin of the basihyal) a triangular space. There is no other cartilage or bone in the anterior arch, unless a very minute pyramidal bone, described by Brandt as articulating with the mastoid process of the skull, represents the stylohyal."

A comparison of the two diagrams on Professor Flower's 179th page, representing sections of the cranium in the young and old African elephant, will show the enormously disproportionate growth of the parietal bones in the adult, and the peculiar cellular arrangement in the diploe of the skull. The analysis of the skull in the Cetacea is very good, although for lucidity it cannot for a moment compare with the memoir by Owen on the genus *Ziphius*.¹ However, Mr. Flower does not repeat the error of the describer of the genus *Belemnoziphius*, who solemnly told us that in a cetacean "the vomer occupies fully a third of the width of the upper face of the rostrum." Such a statement would have seriously damaged the prospects of many a young man preparing for an examination in comparative anatomy. It is hardly necessary to utter the truism that the prefrontals and the vomer are wholly different bones. As on the showing of Professor Flower, in his diagram on page 104, the vomer originates beneath the primitive cartilaginous notochordal base, such an error is inexcusable.

Page 209 gives us a very lucid diagram of the section of the skull of *Thylacinus*, and the author devotes much space to the comparison between this skull and that of the dog. It is significant to those who have based (as did the lamented Dr. Falconer and Professor Flower himself) so much argument on the position of the condyle of the jaw in carnivorous animals, to learn that in *Thylacinus* "the ascending ramus of the mandible is less elevated than that of the dog, the condyle being almost on the same level as the molar teeth."

Leaving the description of the skull in Mammalia, we turn to the analysis of the "shoulder-girdle," arm, and forearm, hand, pelvic girdle, thigh, leg, and foot. In many of his conclusions, the author adopts the views of Mr. W. Kitchen Parker and of Gegenbaur. The following speculations regarding the scapula are new:

¹ 'Monograph of British Fossil Cetacea of the Red Crag.' Palæontographical Society, 1870.

"The greater part of the scapula is ossified by ectostosis (as the shaft of a long bone) from a single centre, which is placed not far from the middle of the bone, but this ossification does not extend into a certain portion of the superior extremity. This part (supra-scapula) either remains cartilaginous, or 'is feebly ossified by one or more endosteal patches, or by the creeping upwards of such deposit from within the main bone' (Parker). When the spine runs out into a projecting acromial process, more or less of its terminal portion is ossified separately as an epiphysis."

The word interclavicle is used for the large T-shaped bone in the *Ornithorhynchus*, considered by Owen as the epicoracoid, while (as far as we are able to make out the not very clear diagram on page 83) the terms pro-osteon and presternum are applied to the episternum and manubrium respectively. The arrangement is most perplexing. The interclavicle (=epicoracoid, Owen) has no homologue in any other mammal.

"This bone differs from the presternum and the small pro-osteal plate behind its lower extremity, as well as the coracoids and epicoracoids, in being developed in membrane."

If Mr. Flower really means by interclavicle Owen's epicoracoid, the above statement is simply unintelligible, but nearly the whole of page 237 is the reverse of lucid.

With regard to the much controverted question of the distinctions between the foot and hand in man and apes, Mr. Flower adopts Mr. St. George Mivart's opinion. The skilful Professor of Comparative Anatomy at St. Mary's Hospital says—

"On account of the ambiguity arising from the as yet unsettled connotation of the terms hand and foot, I think it better in a scientific treatise to disuse them altogether, and to adopt for the anterior extremity (the carpus, and all beyond it) the term *manus*, and for the homotypal posterior segment the term *pes*. The all but necessity for distinct homological terms for such parts is obvious."¹

The distinction of Carnivora into Digitigrada and Plantigrada is, as the author points out, purely artificial, all gradations existing "between the extreme typical plantigrade gait of the bears and the true digitigrade action of the cats and dogs."

Professor Flower very cautiously defines the so-called marsupial bones as follows:

"Nearly all marsupials have a pair of elongated, flattened, slightly curved bones, movably articulated by one extremity to the anterior

¹ Mivart, "On the Appendicular Skeleton of the Primates," 'Phil. Trans., 1867.

edge of the pubis, near the symphysis, and, passing forwards, diverging from each other within the layers of the abdominal parietes. They are, in fact, ossifications in or intimately connected with the inner tendon or 'pillar' of the external oblique muscle, and therefore come under the category of sesamoid bones. They vary in size and shape in different species. In *Didelphys* they are nearly as long as the ilia, while in the kangaroo they are scarcely half the length of that bone. Though largely developed in the Dasyures, in the allied genus *Thylacinus* they are represented only by small, unossified fibro-cartilages.

"These bones are commonly called 'marsupial bones,' though they have no special function relating to the ventral pouch of the female, being nearly equally developed in both sexes, and also in those species in which the marsupium is not present."

The concluding chapter of Professor Flower's work is devoted "to the correspondence between the bones of the anterior and posterior extremity, and the modifications of the positions of the limbs." Two very clever diagrams are given by the author of the correspondence between the surfaces and borders of the scapula and pelvis respectively.

"The principal differences between the shoulder and pelvic girdle of the Mammalia are two:—(1) The rudimentary condition of the inferior or ventral section of the girdle (the coracoid) in the former, as compared with the vast development of the corresponding part of the lower extremity. (2) The free condition of the anterior, as compared with the posterior, girdle. It is neither attached to the vertebral column above, nor does it (except in the Monotremata) join the opposite part in the middle line below. To compensate for this a clavicle is superadded to the anterior girdle in many mammals, for which there is no exact homologue in the lower extremity.

"It has been shown in Chapters XVI and XIX that the terminal segments of each limb present a remarkable general correspondence with certain constant differences. There can be no question but that the carpus and tarsus, the metacarpus and metatarsus, and the various digits, beginning at the pollex in the one and the hallux in the other, are really homologous; the circumstance of the constant absence of one of the bones of the preaxial digit in both fore and hind limbs is most significant."

This last conclusion is fully justified by facts, and has been generally admitted. Such is a brief analysis of this important work. We have already stated our conception of its revolutionary effect on modern osteological science. Its small and unpretending size would claim for it no special attention, but, as the greatest satirist of France said, "Jamais vingt volumes en folio ne feront de révolution; ce sont les petits livres portatifs à trente sous qui sont à craindre." Among such inconspicuous books this by Professor Flower might be ranged, but the very

smallness of the treatise will conduce to its success in diffusing the principles of comparative anatomy throughout the English scientific world.

III.—The Recent Progress of Operative Surgery.¹

ALMOST at the same time new editions have appeared of two of our favorite English treatises on surgery. Sir William Fergusson has brought out a fifth edition of his well-known 'System of Practical Surgery,' and Dr. Druitt's 'Vade Mecum' has reached a tenth issue. As both of these works have been noticed before in the pages of this Review, it is not necessary now to enter into detailed criticism of them. What we propose to do, therefore, in the present paper is, taking occasion from their simultaneous appearance, to glance at the progress which English surgery has made during the last thirty years. In doing so we shall have opportunities of noting how much Sir William Fergusson has done to advance that department of our art in which he has won such a distinguished position, and what good service Dr. Druitt has effected by the sound principles he has inculcated with so much scholarly grace.

To pass in review the progress of English surgery is a very agreeable task. Its long and steady advance has lately proceeded so rapidly that one cannot but regard its present position with satisfaction, and look forward to its future with confidence. In this respect, English surgery may be taken as the measure of surgery in general. It is the pride of the healing art that it is cosmopolitan, that it knows no distinction of country or race. The acquisition of one nation is the heritage of all, the march of one regiment the advance of the whole army. Such being the case, we may well say that what is true in this country is true all the world over, wherever scientific surgery is known and practised at all.

The progress of the healing art may be said to tend in two directions—(1) to supply the want of ready and accurate methods of detecting and discriminating diseases; and (2) to find means of curing these diseases promptly, efficiently, and simply. These two departments—diagnostics and therapeutics—are the heads under which all our progress ultimately ranges itself. Perhaps the most perfect example that I could adduce of the advance

¹ 1. *A System of Practical Surgery.* By Sir WILLIAM FERGUSSON, Bart., F.R.S., Serjeant-Surgeon to the Queen, and President of the Royal College of Surgeons. Fifth Edition.

2. *The Surgeon's Vade Mecum: a Manual of Modern Surgery.* By ROBERT DRUITT. Tenth Edition.

which modern surgery has made in these two directions, and of the precision and simplicity which ought ever to be aimed at in every instance, would be the case of an infantile cataract curable by suction. Here the disease can be detected at once, and with accuracy; its nature is well known, and it can be effectually cured by the introduction of a couple of needles and a minute suction-syringe; the operation can be performed at a single sitting, and the cure may be complete in a few days. I have repeatedly been struck by this proceeding as one of the most brilliant feats in surgery. The pupils of the child's eyes, which were before white, opaque, and unsightly, become, in a few seconds, dark, deep, and beautiful, and the surgeon is literally permitted to give sight to the blind; and this, moreover, not to an aged person, as in the case of a senile cataract, but in one who has the whole term of life before him, and who, but for the intervention of surgery, might spend his threescore years and ten in complete darkness. But it is rarely that such a perfect result as this is granted us; we are generally obliged to be contented with much less. In some instances our knowledge of the nature and conditions of a disease are extremely vague; in others we can appreciate the malady clearly enough, but are almost powerless to remedy it. This is particularly apt to occur at a time like this, when pathology has outstripped therapeutics, and when chemical and microscopical research have made greater advances than our knowledge of drugs and their action. What we have to do, therefore, in any given complaint, is to study its etiology and pathology, to seek to counteract its causes and to apply the antidote to its symptoms as we best can; and the simpler and more effectual such antidote is the better. In estimating the present position of surgery we have to consider these two points, and we shall find, I think, that a great part of the recent progress which we have made consists in improved methods of treating diseases; that is to say, it is therapeutical. The pathology of surgical being for the most part a simpler matter than that of medical cases, the attention of surgeons has not been so much occupied in this department, and their efforts have been directed chiefly towards perfecting the established methods of practice.

In casting our eyes backwards over the course of English surgery, we are soon met by the introduction of chloroform. The application of this anæsthetic stands up as one of the great landmarks in the history of the healing art. Like the important additions which have been made to our knowledge by Harvey, Ambrose Paré, Jenner, Hunter, and Charles Bell, it marks a great step in advance and a notable change in the course of our practice. We shall, therefore, in what follows, limit our

horizon by Sir James Simpson's discovery. We shall not travel much further back than the year 1847, but we shall endeavour to measure the progress which English operative surgery has made within the period which is commonly taken to mark a generation.

If, then, we assign this limit to our essay, the first thing that claims our attention is the application to surgery of what are known as the "modern anæsthetics," and more especially the introduction of chloroform. It would carry us beyond our present purpose if we were to attempt to follow out the numerous modifications which the possession of such an anæsthetic has imported into practice. Suffice it to say that, while to the patient it has been an unspeakable boon, to surgery it has been of the utmost service. As regards the patient, it has not only removed entirely the immediate pain of the knife, but it has mitigated in a great degree the anxiety and alarm with which operations are wont to be anticipated. What it has done for surgery it is less easy to estimate; probably it has contributed with other circumstances to reduce the rate of mortality after operations; certainly it has served to extend our powers considerably. It has made many proceedings which before were inadmissible, not only admissible, but even the established rule of practice. Of this, a striking example—to borrow an illustration from the physicians—is to be found in the cure of abdominal aneurism by pressure. This brilliant achievement, the credit of which is due to Dr. William Murray, could not have been accomplished but for the merciful slumber induced by chloroform. The practice so successfully inaugurated at Newcastle has been speedily followed elsewhere, more particularly in Dublin, till now it is not too much to say that in every case of abdominal aneurism the question of its treatment by pressure must be fully considered. Other examples might be adduced to prove that anæsthetics have extended the domain of operative surgery, and that various remedial measures which before were out of the question are possible now that we can make the patient insensible, but no more striking instance can be given than that just noticed.

But it is not merely in the greater and heroic operations that the benefit which chloroform has conferred upon us is shown. It attends us everywhere in our practice; it is like a favorite tool, ever in the hand of the workman, and if we say nothing more about it here it is only because its uses are too various and too numerous to enumerate in a paper such as this.

But chloroform is not the only anæsthetic that modern chemistry has given us. At the present time the bichloride of

methylene, ether, nitrous oxide gas, and other substances, have each and all their advocates; and, however it may fare with the others, the "laughing gas" seems likely to establish a permanent place for itself. Its peculiar properties make it especially suitable for short operations, such as those of dentistry, and hence there is good reason to think that it will be able to hold its own even against such a formidable rival as chloroform.

Next to the introduction of anæsthetics, the greatest general improvement which has taken place in operative surgery has been in the dressing of wounds. This surgical proceeding forms such a large part of our business that the way in which it is performed cannot fail to have an influence upon the results of our practice, yet surgeons now living can recollect the time when the mode of dressing wounds was complicated and unclearly—calculated rather to retard than to promote their healing. But the old style of "mundifying," "digesting," and "incarning" applications has passed away. It could not stand before the vigorous assaults and enterprising experiments of such men as Liston, Syme and Fergusson. A great change took place. From a complicated system of dressings surgeons passed to one of the utmost simplicity. After having been in the habit of covering wounds with many and various applications, it became the custom to leave them almost entirely exposed. A few folds of wet lint were laid over the wound to protect it from rough contact and to maintain a warm and equable temperature, and underneath this slight covering the natural processes of repair were left to themselves. This system of "water-dressing" has such obvious advantages that it seems to be holding its ground, notwithstanding the rivalry of other favorite dressings. It is simple, cleanly and convenient; it is always at hand, and it admits of various modifications. For these reasons it will probably continue in general use. But great as this improvement is, there is still room for progress. Water-dressing encourages rather than limits suppuration, and suppuration is the parent of many evils. It has therefore been said, "Let us try how wounds will prosper if left entirely to themselves;" and this is the plan which Dr. Humphry, of Cambridge, has carried out with no small success. But this method, though it has the advantage of great simplicity, and of leaving the patient as free from disturbance as possible, does nothing to control suppuration. This evil still remains, and to encounter this is precisely the object Mr. Lister aims at by his "antiseptic method" of dressing wounds.

Suppuration, he contends, depends on the presence of microscopic germs introduced from the atmosphere, and these germs

carbolic acid has the power of destroying. If, therefore, we wash out a wound thoroughly with a carbolized lotion, and then allow no air to reach it except through a carbolized medium, we shall effectually prevent the formation of pus, and we shall "stamp out" those evils which suppuration brings in its train. This doctrine has been ably maintained, and the practice which is founded upon it has rapidly gained ground. During the late Franco-Prussian war carbolic acid was used on a very extensive scale, and it is probable that the experience of the campaign will go far to settle the practical value of Mr. Lister's suggestions. But whether the "antiseptic method" becomes the established rule of surgery or not, there can be no doubt that the introduction of carbolic acid is a step in advance. Its disinfectant properties and its power of limiting suppuration appear to be beyond controversy; and these advantages, be it observed, would remain, even if it should hereafter be proved that the presence of germs is not so intimately associated with suppuration as some are at present inclined to suppose. But carbolic acid is not the only substance with which dressings have been saturated. Chloride of zinc, sulphurous acid, common salt and other materials, have all been used in the same way. Indeed, the whole subject of the healing of wounds has been reconsidered, and among the many advances which surgery has made during the last quarter of a century we must assign a high place to the improvement of dressings.

The next general improvement that we shall allude to is that which has taken place in the modes of arresting hæmorrhage. The attention which has been directed to the dressing and closure of wounds has led surgeons to wish that they could get rid of the hempen ligatures, which not only act as foreign bodies, but also absorb and retain the discharges. To effect this various methods have been devised. Sir James Simpson recommended acupressure. Messrs. Humphry and Bryant have introduced the use of torsion. Mr. Lister employs carbolized catgut, and cuts the ends short off. This last practice, I believe, was first recommended in ovariectomy, and it is one among many of the details which have conduced to make that operation so successful as it is. Whether we use wire or thread made of animal or of vegetable fibre, the ends may be cut off short, with the hope that the noose will either remain inert, imbedded in the tissues, or that it will be absorbed. In any case the rapid closure of the wound is promoted, and we get rid of a piece of twine which may help to keep up an unhealthy discharge. Much as has already been done in what may be called the therapeutics of wounds, much still remains to be

accomplished. This is one of the directions in which we may look for further progress; one of the roads by which we may expect that the healing art will advance.

But it would not be right to leave the subject of general improvements in surgery without speaking of the care and attention which have been paid to hospital construction and clinical arrangements. Perhaps we should not be far wrong if we were to say that the Crimean war, by calling forth the sympathies of the public in favour of the sick and wounded soldiers, and notably the exertions of the late Lord Herbert and Miss Nightingale, had introduced a new era in the economy of hospitals. And not only has increased attention been paid to the subject of hospital construction, but by a careful system of registration the preventible causes of disease are being traced out, and, as far as possible, counteracted; while, at the same time, women of a higher class have been enlisted as nurses, and are regularly trained to their duties. Thus, it would almost seem as if we were only now beginning to recognise some of the elementary principles of our art—the need of sufficient cubic space, of a free circulation of air, of absolute cleanliness, of the avoidance of all specific infections, of skilled attendants, and so forth. These are things which may appear to some to be only remotely connected with the subject, but in truth they have a most important bearing upon the progress of operative surgery.

Before quitting this part of our theme we must not omit to notice another point of progress. It would be unfair upon those who subserve our art if I did not acknowledge the perfection to which our mechanicians have brought the manufacture of surgical instruments and appliances. He is a bad workman who finds fault with his tools, but still a skilful workman may be materially assisted by good tools, and certainly the utmost praise is due to the ingenuity of our mechanicians, the excellence of their materials and the nicety of their workmanship. Without their aid the best planned operations would often be impracticable, for the use of human fingers has its limits, and unless supplemented by instruments their powers would be very limited. The hand would lack the means to execute the most brilliant conceptions of the brain. But such is the skill of the mechanicians that our range has been enormously enlarged. What an example of the successful adaptation of means to an end is a lithotrite! What a delicacy and finish there is about the modern eye-instruments!

So much for the general improvements in surgery, and those conditions which bear upon all cases alike. We shall now proceed to speak of some of the particular improvements which

have been made, some of the new operations or new methods of treatment, which have been introduced during the last thirty years. But surgery has been so active of late, so many new operations have been devised and so many of the old have been modified, that it is no easy task to decide on what to mention and on what to omit. To allude even briefly to all would carry us far beyond our limits. We must, therefore, endeavour to point out those which are the most remarkable, which serve best to illustrate our subject, and which show most clearly the progress of operative surgery.

The special advance which has been made in treating particular classes of cases, will be best illustrated by reference to some departments of surgery. The first that we shall allude to is what has been called "excisional surgery." If excisions and resections are not absolutely a new class of operations, it is at any rate within the memory of living men that they have acquired the status of an established practice in surgery. And if the reader consider how often the question of excision or of amputation arises, and to how many joints excision is applicable, he will admit that too much importance cannot be assigned to the subject. The progress which has been made in this department is in a great measure due to Sir W. Fergusson. It needs only a glance at the various chapters in his 'System' which refer to excisions or resections to see how much he has done to give these operations their present place among the recognised proceedings of surgery. In speaking of one of the largest and most important operations of this kind he says—

"I have ventured to call it the *ne plus ultra* of conservative surgery. I am still of that opinion, for although bolder deeds have been done in removing the whole upper extremity and scapula at the same time, the conservatism of leaving the healthy arm whilst removing the diseased scapula is, in my estimation, the highest reach in that department to which I have given the name, and to which much of the labours of my life has been devoted" (p. 280).

Another department of surgery which has made marked progress during the last quarter of a century is that which is known by the somewhat barbarous term "ophthalmology." The marvellous invention of Helmholtz, which enables us to explore the whole *fundus oculi*, to detect the slightest speck on the retina—nay, almost to read off the condition of the brain itself—is yet only twenty years old. This little instrument, which the surgeon may easily carry in his waistcoat pocket, has produced a radical change in a whole department of our art. It has brought to our view all the deeper structures of the eye, and has enabled us to say with accuracy what is the condition of the optic

nerve; matters about which it was formerly only possible to form conjectures. Having the nerve of sight thus under our observation, and being able to learn a good deal about its state, we can direct our remedial measures accordingly. Of these modern methods of examining and treating the diseases of the eye, Dr. Drunitt gives an excellent account.

In speaking of the ophthalmoscope we are naturally reminded of other instruments of the same kind, and particularly of the laryngoscope. Though various attempts had from time to time been made to get a view of the interior of the larynx by means of mirrors, the introduction of the laryngoscope into medical and surgical practice only dates from the year 1857. But already the aid which it has afforded in the examination of the vocal cords, and in removing morbid growths from their surfaces, as well as in aiding the diagnosis of more remote disorders, is among the most striking successes of instrumental medicine.

Orthopædic and subcutaneous surgery is another department of our art which may almost be said to have come into existence within the period to which this essay is confined. It is scarcely forty years since Stromeyer first suggested and practised the subcutaneous division of tendons in order to rectify deformities, and now there are two large hospitals in our metropolis especially devoted to this branch of practice. An illustration given at page 384 of Sir Wm. Fergusson's 'System' affords an excellent example of what can be done by tenotomy in cases of club-foot. But orthopædic surgery has not confined itself to such cases only; almost every muscle and tendon in the body has been divided for one complaint or another. Some of these operations have not been attended with such successful results as the projectors anticipated, and one or two have been entirely abandoned; still there can be no doubt that tenotomy is a most valuable addition to our means of treating many deformities.

It is sometimes a question whether the diseases of the skin fall under the province of the surgeon or of the physician; but from whichever point of view we regard them, dermatology must be reckoned as another branch of our art which has made considerable progress of late. This is due in a great degree to the improvements which have been made in microscopes, whereby the parasitic origin of many skin diseases has been placed beyond a doubt.

If now we turn from the departments of surgery to groups and classes of disease we shall find that here also we have ground for encouragement.

If we look at aneurisms, which form such an important and

interesting class of cases, we observe that our means of dealing with them have been increased in number and simplified in method since John Hunter's time. That distinguished surgeon and anatomist said, in speaking of operations—

“This part of surgery is a reflection on the healing art; it is a tacit acknowledgment of the insufficiency of surgery; it is like an armed savage, who attempts to get that by force which a civilised man would get by stratagem.”

How notably he helped to advance surgery upon the principle here laid down we all know. By introducing the “Hunterian operation” for aneurism a great step was made towards the yet simpler methods of pressure and flexion which have since been practised with so much success. What a remarkable illustration is this of the progress of recent operative surgery! Truly the civilised man has vanquished the savage! For we can now in suitable cases cure a formidable disease with little or no pain, and without the loss of a single drop of blood, which would formerly have been considered hopeless, or, at the best, would have required an amputation.

If cancer still remains “the mere despair of surgery,” if we are still unable to trace it to its source or to apply an effectual remedy, we may at least affirm that we understand its conditions better than we did. Thanks to the inquiries of Mr. Paget, Mr. De Morgan and the late Mr. Moore, our knowledge of the pathology of this disease has been much advanced.

Another class of cases which has received elucidation of late is that which belongs to the genito-urinary organs. It is but forty years since Dr. Bright threw a flood of light upon those morbid conditions of the kidney which are now associated with his name. It is but forty-five years since the use of the lithotrite became established in the practice of Civiale. These two things, taken together, have done much for the class of cases that we have mentioned. The various morbid conditions of the urine are now known with great accuracy, and their bearing upon medical and surgical practice is tolerably well understood; indeed, we may say that hardly any operation of importance should be undertaken without first ascertaining the state of the kidneys and their secretion. If any one wishes to learn what the lithotrite has done for those who suffer from *calculus vesicæ*, let him refer to Sir Henry Thompson's paper upon the subject in the last volume of the ‘Transactions of the Medical and Chirurgical Society.’ Here, as in the case of aneurism, is a notable example of the triumphs that surgery has recently obtained—the triumph of substituting for a formidable and dangerous operation a proceeding which is accompanied by no loss of blood, and which, in suitable cases, is almost entirely

free from danger. But when we have spoken of kidney diseases and of stone in the bladder we have not enumerated all the progress that has been made in this department. The diseases of the urethra have been more accurately discriminated, and our means of dealing with them have been multiplied, so that we can now do more than we formerly could for those who suffer from stricture and other painful complaints of that canal.

The number of those, both in this country and abroad, who have helped to advance our knowledge of venereal diseases, is too great to allow of our mentioning particular names. But the result of their observations and investigations has been that we now have an improved classification of venereal cases; that we can tell—at least, in the main—which are mere local sores, and which carry with them a systemic infection; that we can apply the appropriate remedy to each without exposing the patient to the risks of a mercurial course, except where there is some definite benefit to be derived from it; and, lastly, that we have still further followed out the action of the syphilitic virus, so that the various ways in which it manifests itself in internal organs are among the latest observations in pathology.

Dr. Druitt has given an excellent résumé of the modern doctrines with regard to the venereal diseases, and of the treatment founded thereon. He has brought to bear on the subject an amount of learning gathered from the writings of bygone generations, and this he has compared with the most recent inquiries as well as with his own experience, and the result is that he has touched a difficult topic with a remarkably firm and delicate hand.

If now we turn from groups and classes of diseases to special operations, the first which arrests our attention is ovariectomy. Though this operation had been long spoken of, it is only within the last twenty-five years that it has been actually practised. Now, thanks to the minute care which has been bestowed upon it by Mr. Spencer Wells and others, the mortality has been reduced within such limits that it has become an established operation, and there can be no doubt that it will hold its place among the great achievements in surgery.

There are several other operations, or modes of operating, which do not fall directly under any of the previous heads, but which ought not to be passed over without mention, though it would carry us too far if we attempted to speak of them all in detail. Of this number are—Teale's method of amputating with rectangular flaps; the amputations through the ankle and foot, which are known as Syme's, Pirogoff's, and Chopart's;

Wood's operation for the radical cure of hernia; the various improved methods of closing fissures of the palate, whether it be by operation or by obturators; more efficient means of dealing with varicose veins and hæmorrhoidal tumours; the transplantation of skin, which has lately been introduced to the notice of English surgeons by Mr. Pollock, and the hypodermic injection of medicines.

We have had also numerous additions to our armamentarium, some of which ought to receive a passing notice; for example, the wire suture, the starched bandages, the *écraseur*, the galvano-cautery, the "caustic arrows," various cradles and swings for the limbs, and many other appliances which deserve to be enumerated.

In this sketch of the recent progress of surgery, it would have been interesting, if our space had permitted, to have inquired how far the various improvements mentioned are novelties, and how far they are merely the revival, under more favorable conditions, of modes of practice long since suggested. Certainly it would not be difficult to show that some of our latest proposals were anticipated by Hippocrates and other ancient writers. But to consider this question fully would require a separate essay, and it is now time for us to draw towards a close.

The area over which our view has extended has been very limited. We have been like a traveller who ascends some mountain peak or some cathedral tower, in order that he may take the bearings of his journey. Beneath him he sees the town or the village spread out at his feet, and a little further off he observes the hamlets, the woods, and the streams, stretching away in the distance, but beyond that there are only lines of dark purple melting into blue, till the eye reaches the far off horizon, where earth and sky appear to be blended. It is only in the foreground of the landscape, in the circle immediately around him, that he can take note of individual objects. So has it been with us. We have glanced at surgery, but only at recent surgery. We have not attempted to enter into the history of the art. We have not occupied ourselves with distant centuries or remote generations. We have confined our observations to less than a half century, in fact to a single generation.

But, limited as our survey has been, it may well fill us with hope. The achievements of the past augur well for the future. The successes which surgery has had during the last few years give us good ground for anticipating still further conquests. For who can set bounds to the development of an art which is rooted in the fruitful soil of scientific truth? With public opinion

awakening to its value, with the sciences upon which it rests making the most remarkable progress, and, above all, with an unparalleled activity within the medical profession itself, what may we not anticipate? Much has been already accomplished, and it is probable that what has been done will serve as the basis for yet more rapid progress.

“Others, I doubt not, if not we,
The issue of our toils shall see—
Young children gather as their own
The harvest that the dead have sown—
The dead forgotten and unknown.”

But to attempt to indicate the future course of surgery, or to define the limits of its progress, would be hazardous in the extreme. It is always hazardous to say what can or cannot be done. It was said that steamships could never cross the Atlantic, nor railway trains travel more than twelve miles an hour! How far such assertions have been verified we all know. We will not, therefore, venture to forecast the future of surgery. But this we may safely affirm—that it is yet only in its infancy, that there is within it a latent power which patient toil cannot fail to bring forth, and that it has yet many rich blessings in store for suffering humanity. To its votaries we may well apply the words of the laureate:

“Men, my brothers; men, the workers; ever reaping something new,
That which they have done but earnest of the things that they shall do.”

And what, in our case, is this work? To put strength in the place of weakness, health in the place of sickness, and life in the place of death—this is the end of all our labours, and to bring about this we may well be contented to work with patient toil. That our efforts have not been in vain, that, if much yet remains to be done, much has been accomplished even within the last thirty years, this brief sketch sufficiently testifies.

IV.—Electro-therapy.¹

If the advances of the art of electro-therapeutics could in any degree be measured by the multitude of its professors, and by

¹ 1. *Elektrotherapie*. Von Dr. MORIZ BENEDIKT. Wien, 1868. 8vo.

2. *Die Electricität in ihrer Anwendung auf Practische Medicin*. Von Dr. M. MEYER. 3 Aufl. Berlin, 1868. 8vo.

Electricity in its Relations to Practical Medicine. By Dr. MORITZ MEYER. Translated by Dr. HAMMOND, New York. 1869. 8vo.

the profusion of essays and treatises which issue from the press, we should have more reason for congratulation than at present is permitted to us. Not so much, perhaps, in England as on the Continent, and on the Continent in Germany chiefly, there now exists a vast mass of electro-therapeutic literature, the quality of which bears no kind of proportion to its quantity.

The books at the head of this article are a small selection from the heap before the writer, and from the still greater heap which is not before him, the former consisting; like the latter, of a few large and serious books, a number of lesser works of various degrees of merit, and a great collection of pamphlets and magazine articles. These are, as I have said, chiefly German, and were it not for the treatise of Dr. Althaus England could not claim to be even the foster nurse of the art. Many English writers, such as Dr. Radcliffe, have incidentally done much for electro-therapeutics; we have also produced a few articles in the journals, such, for instance, as the recently published lectures of Dr. Russell Reynolds; but we have to thank Dr. Althaus for the only important original work which has as yet appeared in our own country. I venture to hope, therefore, that the author of this laborious and excellent treatise will allow us to claim him as a compatriot. All the works here cited are, with the exception of that by Fieber, large and important treatises, as a selection from the lesser works seemed too difficult to be worth making. I feel, moreover, the less to blame for any omission of these, as Dr. Althaus' wide reading has enabled him to notice all the more prominent of them in a fairly adequate way up to the date of the publication of his volume.

Fieber's 'Compendium' I have cited for a special purpose—for the purpose of saying that as yet a compendium of electro-therapeutics is not possible. While on some of the most fundamental questions of the science the best observers are at variance, it is absurd to pretend to lay down the principles of the art for young students or for medical men whose experience lies in other directions. As yet our ignorance of the morbid changes of the nervous system and of its peculiar susceptibilities, together with a correlative ignorance of the exact conse-

3. *Untersuchungen u. Beobachtungen auf dem Gebiete d. Elektrotherapie.* Von RUDOLF BRENNER. Bd. I, 1868. Bd. II, 1869. Leipzig. 2 vols. 8vo.

4. *Compendium d. Elektrotherapie.* Von FIEBER. Wien, 1869. 8vo.

5. *A Treatise on Medical Electricity.* By JULIUS ALTHAUS, M.D. London, 1870. 8vo.

6. *A Treatise on Localized Electrization.* By Dr. G. B. DUCHENNE. Translated and edited by Dr. HERBERT TIBBITS. London, 1871. 8vo.

7. *Medical and Surgical Electricity.* By Drs. BEARD and ROCKWELL. New York, 1871.

quences of some of the simplest procedures in electric application, make it quite impossible to construct a grammar. Add to this the threatening truth that electricity in the hands of unlearned persons is an edged tool of a most dangerous kind, and we shall not hesitate to condemn any one who pretends, as yet, to write a handbook for the encouragement of tyroes. For my part, I never see a student working his own will with a battery upon a patient without the most anxious forebodings. Not only may such terrible consequences as loss of sight or cerebral hæmorrhage result from a careless application of the battery current, events which even the readers of Duchenne and Benedikt must, however, hope are exceptional, but a disease treated in an improper or an untimely manner may be aggravated rather than bettered, or, to go still further, may suffer harm from the treatment, however appropriate in kind, if it be applied carelessly and without management. For these reasons Fieber's book is, in my opinion, calculated to do more harm than good. Regarded from the point of view of the more advanced inquirers, it is both incorrect and inadequate, and it can only mislead the students who put their trust in it. These remarks seem necessary lest we should see, as we may expect daily to see, a number of writers hastening to publish such handbooks in order to connect their names with a fashionable remedy. Even in the more serious writings some increase of the spirit of true caution is to be desired; and as arguments and counter-arguments are more and more fairly stated, and conclusions are set forth with less of confidence and more of saving clauses, so must comprehensive books become as yet rather larger than smaller. These considerations make me unwilling to bring relief to the reader's mind by assuring him that a great part of the electro-therapeutical publications under which our shelves are groaning is of no great value. I might even whisper to him the consoling secret that much of all this, though written in German, is, nevertheless, rubbish, and that if we collected the wheat from the tares we should bring our materials into some reasonable compass. Fortunately, however, oblivion will scatter her poppies over much of it, and time with hard work will preserve what is good, leaving the useless to be forgotten.

The works of Althaus, Benedikt, Meyer, Brenner, and Duchenne, to which I should like to add Ziemssen's book also, though this last is not so recent as the former, have in them the results of too much earnest labour and intelligence to pass quickly away. Whatever faults may be found in them, and whatever shortcomings, they do, nevertheless, represent an amount of work and thought which is most honorable to their authors, and full of value, both for the present and the future.

This tribute is especially due to the veteran Duchenne, whose untiring energy and ready ingenuity have produced results which will make him immortal as the originator of "localized electrization." If on some points of scientific argument we are disposed to charge M. Duchenne with error, those who, like myself, have been his pupils, who have followed his method, and witnessed his quick perception and adaptation of means to ends, will never forget that his practical teaching is, on the whole, as truthful as it is vivid, and will contend that few great explorers of the thorny ways of therapeutics will leave work behind them more likely to be of permanent value and of benefit to future generations.

Duchenne's original work on localized electrization was so well known to all workers in the matter, that the long postponement of the translation is not altogether to be regretted. The interval has been full of activity, and the instalment of the English edition, which Mr. Radcliffe's engagements have obliged him to entrust to his successor, promises that the results of the best subsequent work will be incorporated into Duchenne's own treatise with so much judgment and adequacy as to make this edition an independent standard.

We have to congratulate Dr. Tibbits on his successful labours thus far, and to express our hope that, the issue being now begun, he will not allow us to wait long for the completion of his work. It is well known that the strength of Duchenne's method lay in his abandonment of the loose applications of former electricians, and his accurate determination of its action upon special tissues and upon special parts of the body. It is not too much to say that the happiness of his results was in full proportion to the well-directed labour and indomitable perseverance which were the means of their attainment. The unbridled license of the earlier electro-therapeutists brought the whole subject into discredit—an undeserved discredit, from which it is now hardly recovering; and we must hope that more recent investigators will profit from this lesson of the past, and will pursue their study with that closeness, caution and temperance, which alone can ensure success. We may admit that the art of electro-therapy must precede in some measure the science upon which it must ultimately be based, but this gives us no excuse for random reasoning or for loose and irregular observations. I wish that the works of the writers before us could be held altogether blameless of these faults of method. But to this we must return. In especial reference to Dr. Tibbits' edition of Duchenne, no such reflection can be made. The tentative and accurate spirit of the master has found its kin in the spirit of his follower, and we see little in the work of either which is open to cavil or to correction. The

present volume opens with some general observations upon the various forms of electricity used in medicine. The first form, static—or to use Mr. Radcliffe's term, Franklinic electricity—the electricity, that is, which is developed by friction and established in Leyden jars, has not received any wide application in medicine, save in the so-called electric baths, when the patient is himself turned into a Leyden jar. These "baths" do not change the relative equilibrium of the several parts within the sick body, and are probably quite useless. The use of the spark seems, however, to have been used at the Hospital in Queen Square by Dr. Radcliffe for the treatment of facial spasm (tic convulsif), and with striking results. Dr. Radcliffe has also found the spark useful in hysterical aphonia.* The induced currents for the purposes of local application are, of course, Duchenne's great means of cure; this kind is now known as faradic electricity, after its great discoverer, and its use is known as faradism. The continuous current is distinguished as galvanism, or 'voltaism' (Radcliffe); the term galvanism being the more popular, and more likely, therefore, to remain in general use. It is much to be wished that medical men would confine themselves to the more accurate use of these terms. We constantly hear of patients being submitted to galvanism when their treatment has actually been faradism, the confusion being a serious one, as the two modes of treatment are totally different. By galvanism, then, we are to mean treatment by the continuous current, and treatment by the interrupted or induced current is to be distinguished as faradism. In his second chapter Duchenne describes in temperate words the imperfection of the operative proceedings in vogue when he commenced his researches. The fact really being that, before Duchenne, what was called a system of electro-therapeutics was not in existence, and the procedures which claimed that title were of the most indefinite and delusive kind. Duchenne endeavoured to bring some order into this chaos, and set himself to find out clearly what he wanted to do, and how to adapt his means most accurately for the attainment of his wants. He first inquired how to direct electricity through organs, and how to set limits to its action; thus he arrived at the now well-known property of moist conductors to carry their influence through the skin into the underlying parts, and of dry conductors to limit their influence to the dry skin upon which they are placed. Thus he found himself enabled at will to act upon definite muscles below the skin, or to restrain the stimulant to the skin itself in cases of anæsthesia and the like. Another most important and useful

¹ See Dr. Radcliffe's 'Lectures on Epileptic and other Disorders of the Nervous System.'

fact which he then discovered was this—that as a general rule the electric excitation applied to a muscle is for the most part localized in the muscle, and especially that it does not penetrate from the muscle up to the nervous centres, and thence set up reflex agitations. It is obvious how important is this limitation for successful practice.

In all the books before us the various instruments used for therapeutical purposes are described very fully, and by no one more fully than by Dr. Althaus.

In this complete volume the English reader will find an adequate account of the science of animal electricity, or rather of what is known of it, and also a careful description of all the more important apparatus which has been used for therapeutical purposes. It would lead us far beyond our present limits were we to enter upon any such descriptions in this place; but the choice of an instrument is so important that we cannot, in justice to the reader, pass this subject by. Fortunately the medical profession in England is tolerably well agreed upon the merits of the instruments I am able to recommend. Duchenne's battery for induced electricity—a modification of Dubois-Reymond's instrument—is, perhaps, the best for medical purposes, but it is very cumbersome. In America, as we learn from Hammond's translation of Mayer, and from the treatise by Beard and Rockwell, the American makers have devised instruments which seem, and no doubt are, fully adequate in every way. In England, however, we are all well satisfied with the instruments made by Stöhrer of Dresden, and sold by Pratt, of Oxford Street. They are handy, portable, and remain in order for a long time. Stöhrer makes instruments of two sizes; the larger instrument having, however, the advantage of the smaller, not in size alone but in many details of its construction. In the larger instrument are two zinc-carbon cells which may be used separately or together, but the means of combining and separating are not so ready as they might easily be made. When not in use the elements are, by an ingenious contrivance, removed from the solution, so that their decomposition is long postponed. In the other half of the box are the two coils and the rheotome. The coils in the larger instrument are very conveniently arranged for purposes of graduation. The primary coil is graduated by the elevation of a copper sheath, and the secondary coil by its partial or complete exposure to the influence of the primary. The rheotome is simple, easily managed, and does not get out of order in the capricious ways which are so annoying in some instruments. Its drawbacks are that there is no means of reversing the current without removing the electrodes, and there is no means of changing from the primary to the secondary current without unmounting

the conductors. The rheophores again are, in my opinion, of a very bad form for practical purposes, a defect which is easily remedied. Rheophores are wanted which have holders large enough to receive good-sized sponges, and which can both be held in one hand, the other hand being thus released for the purpose of graduating the currents. There should also be a better electric brush, and one or two conical rheophores provided for application to facial, manual and other small muscles and nerves. Of the continuous batteries we have also a large choice, but here again in England we have all agreed that the best battery is that known as the Becker-Muirhead, supplied by Messrs. Elliott. It consists of a hundred of modified Daniell's elements, and answers well in practice, remaining long in force even when regularly in use. As supplied by the makers without galvanometer, rheotome, or any additional parts, its price seems very high, and the addition of one of Becker's galvanometers is absolutely necessary for careful work. The battery has, moreover, the drawback of being fixed, so that, although it is suitable for a consulting room or out-patient room, as Dr. Althaus uses it, and as we use it in the out-patient department at the Leeds Infirmary, yet in ward work and under other circumstances where a portable battery is required it is not so useful as the beautiful little instrument made by Foveau, and supplied by Weiss and Son. The instrument consists of fifty cells only, but these are said to be equal in intensity to the 100 cells of Muirhead. I have lately replaced an old battery by this made by Weiss, and so far I have every reason but one to be highly satisfied with it. The one slight defect is the want of a stout sunk handle in the lid for the purpose of easy and steady carriage. By an ingenious lifting arrangement the cells are removed from the acid during the time the instrument is not in use. In this way the drawback of continuous corrosion is counteracted. With Stöhrer's induction instrument of two cells and Foveau's continuous battery the practitioner will do best to arm himself, and these instruments will require less attention from busy men than most others. Drs. Duchenne and Tibbits give a good account of these instruments and of many more, but the arrangement of the volume is very far from perspicuous, the voltaic instruments being described in the second chapter, while the induction instruments are described separately in the fourth, which is the last in the first volume. The third chapter is devoted to a description of the various methods of electrization, such as electro-puncture, electrization by reflex action, localized faradization, and the continuous currents. In this chapter a great deal of illustrative clinical matter is inserted which would come more fitly into sub-

sequent chapters. There may be some original conception which has governed the development of the treatise, but I have not managed to place myself as yet at the right point of view, so that its parts appear to me as yet to be in a state of confusion. In this respect Dr. Althaus' work seems to have the advantage. Dr. Althaus follows the more intelligible plan of describing first the modes of electricity in inorganic and in organized bodies. In the following chapter he deals at length with electro-physiology. The author here shows a thoroughly earnest desire to investigate as far as possible the action of electricity upon the various tissues in a state of health, which must be the basis of all therapeutics. This chapter seems fully up to the mark of recent knowledge, though it is only when our recent knowledge is thus put together that we see how limited and contradictory our conclusions are. To name but one out of many obscurities, the contrasted effects of different degrees of electrization are yet to learn. That small doses of the agent may present consequences different to those of large doses, that, indeed, they generally present opposite consequences, is probable from what we know of such experiments upon single exposed nerve-trunks. If we take the other extreme case of a cerebral hemisphere, we have only just been assured by Erb that a current applied to the cranium influences the brain at all, and as to the various and probably contracted effects of different doses as yet we know nothing. This is but one instance of the numerous difficulties which meet us on the very threshold of the subject, difficulties which are not so clearly set forth, nor, indeed, so fully recognised, by Althaus, Benedikt, and Meyer, as would seem desirable. We cannot call upon an author to illumine all the dark places of his study, but we must desire to see a clear line drawn between the known and the unknown. Althaus in his fourth chapter deals with electricity as a means of diagnosis, and enters intelligently into the contrasted phenomena of peripheral and central palsies, and of those which, like hysterical palsies or progressive muscular atrophy, lie on the confines of the two classes. In this chapter Dr. Althaus seems to me to be scarcely alive enough to the undoubted fact that the same disorder—such, for example, as a striatum hemiplegia—may, and often does, present wholly different degrees of reaction to each kind of electrization at different stages of its own progress. I have a case of common left-side hemiplegia in my mind at this moment which was not treated by regular electrization of any kind, but which presented various susceptibilities to induced and voltaic currents respectively at various stages of its course. At one time it responded to voltaism exclusively, at another

time to faradism chiefly, and so on. It is equally important to point out that renewed activity to an induced current is not always attained in proportion to the diminution of sensibility to the voltaic currents, and we may often see both these irregularities in the well-studied test disease of Bell's palsy. Of striatum hemiplegia Dr. Althaus has examined one hundred cases from the present point of view, but he does not give, what is quite of equal importance, the results of a series of one hundred test applications upon one case. When to this difficulty we add the now generally admitted fact, first shown, I think, by Erb, that the results of direct nerve stimulation may be different from, or quite opposite to, those of direct muscle stimulation, we see how my remarks as to the use of the remedy in unpractised hands are justified.

In Chapter V, a long and complete treatise in itself, Dr. Althaus gives us the results of electro-therapy as gathered from the writings of others, and in great measure also from his own experience. It is, perhaps, inseparable from such a discussion that the author should relate a large number of cases with conjectural diagnoses; this fault, whether inherent in the author or in his subject I can scarcely decide, is also painfully evident in Benedikt's volume, and we are thereby prevented from attaching a permanent value to many of the cases recorded. In Althaus, and still more in Benedikt, I find many cases adduced as evidence, the exact nature of which is to me very far from evident. Another defect, and one of which we shall be less tolerant, is the absence in great measure of negative and unsuccessful cases. There is, for this reason, in Dr. Althaus' book just that flavour of the advocacy of a nostrum which to a candid and cautious mind is slightly repulsive. To me no good purpose seems to be gained by stringing rows of cures together; what we lack is series after series of cases with their results for good, for nothing, or for evil. It is not that Dr. Althaus prints no unsuccessful cases, still less that I accuse him of suppressing cases, but he will be the first to admit that he has selected favorable cases, as a rule, simply as such. Is it not the most scientific, and in every way the best plan, to give a series of cases as they stand in the physician's note-book, giving them with their outcome of whatever kind, the unfavorable having their lesson for the physician as much as the favorable? No one could do this better than Dr. Althaus, whose experience in all kinds of cases suitable for electrization must be very large. He would help us very materially if he would publish a tabulated synopsis of his experience in any one or more classes of disease, failures and successes being exposed alike. As it is, we can scarcely help the feeling that the book is at present

the work rather of an advocate of electro-therapy than of a judicial investigator of its claims. We miss, moreover, in the volume before us that organizing power which so co-ordinates facts that the whole is represented to the reader in a vivid and impressive way; this power is given, however, to a few only, and we have no right to require it from all. As men vary, so various are their gifts. Industry and competent experience we do require, and in industry and experience Dr. Althaus reaches a high standard. It has been said that the highest compliment which can be paid to a man's arguments and intelligence is to differ from him. I hope Dr. Althaus will receive the assurance that in this spirit only have I ventured to qualify the expression of praise and welcome with which we must accept at his hands this new edition, which is, in fact, a new work.

Benedikt's work is one of which it is very difficult to give an account within narrow limits, for it is a book whose great merits are diminished by faults almost as great. My own impressions of the book have followed the course which I think it is likely that they have taken in other readers. Here is a writer who has devoted himself to his subject with a quiet enthusiasm and a steady unconquerable perseverance which are rarely seen anywhere but in those men who, being German, have also been trained in the thorough methods of the German schools. The first chapter deals with the electric instruments, and Benedikt is right in avoiding the prolixity with which many such chapters are written. A complete account of all instruments which have met with a certain degree of acceptance is very desirable, and it is well that some one should write such an account, and write it as fully as possible. Benedikt calls his book, however, a '*Lehrbuch*,' and it is certainly nothing else than a weight and a bewilderment to the student to have to wade through pages of woodcut and description which have no practical importance for him at all. All that such a book should contain is an account of the instruments which the author recommends to the reader, with just so much, and so much only, of physical science as is required for his instruction in the use of them. The second chapter deals with the physiology of the subject, and the third and fourth chapters with methods of experimentation and treatment. Benedikt then divides the remainder and chief part of his treatise into—(A) neuroses from a symptomatic standpoint, and (B) neuroses from a pathologico-anatomical standpoint. Part A, for example, contains neuralgias, anæsthesias, motor irritations, motor palsies, inco-ordinations, trophesias, as Dr. Laycock's term is, and reflex neuroses. The part B contains cerebral and cerebellar diseases, diseases of

encephalic nerves, spinal diseases, general neuroses (hysteria, &c.), the neuroses of fevers and poisons, and peripheral or local diseases, such as rheumatism, injuries, bladder diseases, and so forth. This arrangement is in conception by no means a bad one if very cautiously carried out. It requires, however, a superhuman degree of caution, as many loopholes are left for vaguenesses of every sort and kind. On the other hand, it is hard to avoid frequent repetitions in the second part of things set forth in the first. On the whole, however, I do not know that Benedikt has injured his work in any serious way by this mode of arranging it. Its faults are inherent. The course of my own impressions has been, I am sorry to say, from the more to the less favorable, though it is with great diffidence that I venture to express an adverse opinion of a work by an author in comparison of whom my experience is as nothing, and of one, moreover, which has obtained so favorable a reception in England. At the same time, however great the experience of the author, and however great his fame, he is none the less liable to judgment in matters of reason and method, and on these grounds his work seems open to very grave question. Benedikt opens his volume with a series of aphoristic deliverances, which are irreproachable in tone, and lead us to form the highest anticipations of the results obtained by so excellent a preacher. The importance of case histories is set forth; we are besought to confine ourselves to legitimate induction, the true method of which receives a numerical illustration; we are warned not to allow preconceptions to interfere with the simple impression of facts; we are taught duly to distrust analogical reasonings; statistics are pitfalls of the most treacherous kind, and so forth.

All this we have heard before, though it cannot but be good for us to have such sentiments clothed once more in imposing language; at the same time we cannot help looking for the examples which are to follow these unexceptionable precepts, and we are disposed to measure the author himself by the measure which he metes withal. Tried by his own admirable standard, Dr. Benedikt proves his energy and perseverance, his extensive experience, his collection of a great mass of useful material, and of many valuable reasonings thereupon; but if we try to set these reasonings into something like systemic order, if we try to compare his facts with his inferences, nay more, even if we look no further than the certainty of his method, we shall find some suspicion slowly creeping upon us that all is not right, and as we read on we shall become convinced that the author's method is not safe, that his inferences are not close, that his assertions are not coherent, and that his book, as

a whole, so far from being a trustworthy guide to the student, is more likely to do him harm than good. I am now bound to show some cause for these somewhat sweeping assertions, but within my present limits it is, of course, impossible for me to prove my objections at any length, or, indeed, in any systematic way. The following remarks I shall set down something after the manner in which they first presented themselves to my mind. The first doubt which crossed my mind was caused by the uncertain, I might almost say the reckless, diagnoses attached to the cases so numerous recorded in all parts of the volume. I am quite sure that any reader familiar with nervous diseases, whether familiar with electro-therapy or not, who will take up Benedikt's book and run his eye over any score of his obscurer cases, will lose his breath when he reads the diagnoses which in unhesitating confidence are attached to them. Not only are diagnoses of central diseases made in the most airy manner, but states of nerve are predicated without anything like adequate reason. The confidence with which this is done is often so great as for the moment to disarm the reader's criticism.

There is no more remarkable instance of this than in the section on local or peripheral palsies. Benedikt fails utterly in representing clearly, even to himself, the distinction between progressive muscular atrophy and local palsies, such as palsy of the extensors or palsy of muscles supplied by one nerve, such as the ulnar, &c.; and, again, between these and writers' cramp, which is now generally known to differ from both in being a disorder of co-ordination—an ataxy. If any one has made these palsies his own, and has, by acuteness of observation and precision of description, distinguished them one from another, that teacher is Duchenne. Benedikt, in wilful neglect of Duchenne's very words, makes, first, an error of diagnosis, and then endeavours to confute Duchenne by his own false instance. Duchenne says distinctly that the cardinal mark of progressive muscular atrophy is this—that the muscles in this disease lose their motor power and their reaction to faradism in direct proportion to the degree of their atrophy. Can anything be plainer, then, than this—that any disease not obeying this rule may be what it pleases, but certainly is not progressive muscular atrophy. When, therefore, we find Benedikt revising Duchenne's views, asserting that the loss of reaction does not observe this direct relation, and producing cases to show that the electro-muscular contractility is sometimes even normal, it would have occurred to most men that the difference might lie in the cases compared, and that it was not Duchenne's definition, but Benedikt's diagnosis, which was at fault. At any rate, the reader will not hesitate for a moment in accepting the

latter alternative. There is no doubt that the cases in which the loss of motor power and the response to faradic excitation were not in proportion to the atrophy, or the atrophy to them, were mere local palsies, and not cases of progressive muscular atrophy at all within the accepted limits of that term. Another evidence of the lack of anything like the instinctive accuracy of the truly scientific observer is seen in Benedikt's rash assumption of power over certain nerves. Those of us who are conversant with Duchenne's extreme care and minute caution in these respects will be astonished to find Benedikt laying down the law, first, that nerves are, of all the tissues, most sensitive to electric excitation, and assuming, therefore, whenever he applies his rheophore in a nerve district, that he is acting definitely upon that nerve, no calculation being made for the degrees of conductivity possessed by intervening tissues. The assurance with which Benedikt assumes that a rheophore placed behind the jaw influences the sympathetic, and through it the nutrition of almost all the bodily organs, is perfectly amazing. No cautious observer can be satisfied as yet that we have any means of acting freely and directly upon the sympathetic nerve at all. I, who have given some attention to this point, am disposed to deny that we have any such means, though I should hold it as unscientific, at present, to deny it as I do to assert it. This being the opinion, not of the writer only, but of many of the most competent therapists of the day, how are we to excuse an author who piles cases upon cases of cure by means of galvanizing the sympathetic, the cures being intended, I suppose, by a *ὑπερὸν πρότερον*, to prove that the sympathetic is thus easily reached? Here, of course, we come to a question of fact, and such facts and such cases as I possess are against the hope of attaining any important results by galvanizing the sides of the neck. Nothing can well be more wearisome to the careful and tentative student than this endless way of turning all difficulties over to be dealt with by some supposed action of the sympathetic. We cannot deny, and we have no purpose in denying, that the sympathetic nerve has important offices in the body, but the less we know of it the more useful it seems to be as a refuge for those destitute of an hypothesis. But to make the hypothesis first, and then to build a whole department of therapeutics upon it before it is proved, seems, indeed, like balancing the pyramid upon its point. Here is a neck, and inside the neck is a sympathetic nerve system; if we galvanize the neck the influence will flow somewhere about among the tissues, according to their various conducting powers, their relative positions, degrees of moisture, and other physical conditions; therefore the sympathetic may, probably, come in for

some of it, and we all know that a galvanized sympathetic causes contraction of blood-vessels. Now, there are blood-vessels all over the body, and in the brain as elsewhere, so, perhaps, they will be kind enough to contract among others, and, perhaps, contracting the blood-vessels in the brain may do the brain some good, though how is not quite clear. Meanwhile, if we see no changes in the pupil, ear, or cheek, or but slight changes only, we must have faith and hope that more sensible changes are in progress within! Surely, this is the very mid-summer madness of reasoning, and yet it is no unfair rendering of the arguments used largely by Dr. Benedikt, and, I must add, by Dr. Althaus also, though with more caution. Now, how do the facts lie in the minds of careful physiologists? Four times, at least, have special inquiries been made into this question—by Callenfels, in 1855; by Northnagel, and again by Schultz, ten years later; and by Riegel and Jolly, in Von Recklinghausen's laboratory, in November, 1870. Of all these observers, Callenfels is the only one who describes changes in the vessels of the pia mater as the consequences of section or irritation of the cervical sympathetic. Schultz denies that any such consecutive changes are to be seen; Northnagel generally confirms this statement, but thinks they may be noticed in exceptional cases; and finally, Riegel and Jolly, after a long series of observations, state that in no single instance, when the experiment has been carefully performed, have they been able to detect any change whatever in the vessels of the pia mater, whether they simply cut the sympathetic or, in addition, they irritate the central extremity. So that, in the first place, it is not only doubtful whether the application of a rheophore in the neck has any definite effect upon the sympathetic at all, but, in the second place, it is clear that if it be affected such affection has no consequences in the superficial vascularity of the brain whatever. And yet a great part of Benedikt's volume consists of a system of therapeutical procedures based upon such a foundation as this. As regards the central vascularity of the brain, it is conceivable that one who aims at the sympathetic may really influence the rich innervation of the carotid, and may thus interfere with the inner cerebral arteries. We have no evidence, however, to tell us whether the symptoms which follow an application of strong currents to the head and neck are due to this, or rather to irritation of sensitive surfaces with "reflex" contraction of the vessels, or rather, again, to the direct penetration of the current with all the parts disturbed. Surely the preliminary difficulties must be taken in hand before any system of therapeutics can command our confidence.

Take reflex actions again: how easy it is to set down certain observed results as reflex actions! The extensors in a case of lead palsy are galvanized, and we have a contraction in some of the healthy muscles of the arm, synergic or opponent, as it may be, and without making any adequate inquiry into the mode or degree of the diffusion of the influence within the tissues of the limb itself, we are to take the less troublesome course of referring all such indirect reactions to "reflex action." This Benedikt does constantly, and in the most innocent manner possible. Now, Duchenne has made a series of minute and convincing inquiries into the means, if any, of producing such reflex movements, and his results are in direct discouragement of this easy method of accounting for the contraction of muscles other than those intentionally included. As to the wonderful "diplegic contractions" so much talked of by Remak, and coquetted with by later writers, it is quite refreshing to read the simple straightforward facts as they are recorded by "men whose eyes are open." Dr. Tibbits (*loc. cit.*, p. 222) says simply, "As yet, after the most careful observation, and equally careful and very necessary endeavour to exclude sources of error, 'diplegic reflex contraction,' so called, has never been observed in the practice and experimental researches at the Hospital for the Paralysed." So much for these phenomena which have exercised so many observers, and which we at Leeds also have sought for in vain. As to the simple and ready use of other "reflex" interpretations, it seems probable that they are to be used with caution, and only apply in exceptional states. Duchenne's experiments are to be read at page 42 of the English edition, where he asks himself, "Is it not to be feared that the electrization of a muscle may provoke, not merely its own contraction, but also (by reflex action) that of other muscles?" If this be so, he says, localized electrization is a chimera. After his experiments he concludes, "that reflex contractions are produced during electro-muscular excitation only in certain pathological conditions, &c." The last adverse criticism which I have to make upon Benedikt's book is one which I owe entirely or chiefly to Brenner, and it is, perhaps, the most important of them all. It is on the fundamental question of method. The readers of Benedikt must have been discouraged by the frequency of a cumbrous terminology, a terminology destined to indicate the series of tissues through which any particular current has passed. If such a terminology be necessary, the aspirant to electro-therapeutical lore may well be dismayed, and may well abandon it to specialists. Fortunately it is not either necessary or in any way desirable to talk in this language, but rather both useless and misleading. It is not for me, or, perhaps, for any one far

more able and experienced, to blame an author for the adoption of a method which has commended itself to him after a long investigation. If it is wrong, let it be replaced, but meanwhile the author has done his best. The fault is not a fault like the inexcusably loose inferences which we have been discussing heretofore; at the same time it is not easy for a plain mind to see why the passage of a current from the hand to the cervical spine, let us say, had such a virtue when passed downwards, and such another virtue, or such a negative or harmful action, when passed in the opposite direction. If we took a midway part, that part, it would seem, could not care much which way the current went so long as it was included in the circuit. Even as regards this latter point also, many of us felt that when two poles were placed apart the current between them would wholly disregard Benedikt's grammar, and instead of behaving itself as an ascending or descending "Spinal root-Nerve stream," or "Spine-Plexus stream," and the like, would simply take the path of least resistance. Now, that this path coincides with our anatomical preconceptions may be true or may not be; nature is not generally so accommodating, and Benedikt seems to have made no adequate calculation whatever of the various resistances of tissues, which are not only various in structure, but which vary infinitely in relative position, in degree of moisture, in thickness, and in many other physical conditions.

So many of us were prepared to receive the new method of Brenner, in recommending which Brenner points out, what we suspected, that the difference of application lies, not in ascent and descent, but in the different action of the two poles. Brenner points out, in a way which is not only clear enough at first sight, but in a way which has justified itself even to his adversaries, that the negative pole differs from the positive, not only in thermal and chemical, but also in physiological properties. It follows, therefore, that to apply the positive pole to a certain region is a totally different thing to the application of the negative pole. Upon this basis Brenner bases his "polar method," a method which will certainly supersede that of Benedikt. I must, however, say once more that, although I have been obliged in the interests of truth to express myself somewhat strongly upon certain defects, as they appear to me, in Dr. Benedikt's work, yet, on the other hand, in the interests of courtesy and fairness, I must repeat that the work is the evidence of long and earnest labour in this department of medicine, and that although it is vitiated throughout by falsities of thought and method, nevertheless, to one who is duly on his guard against these dangers the book will prove a rich mine of experience. Brenner's volumes are far pleasanter matter for the critic

Educated in the application of electricity to a very limited district, namely, to the ear, and being naturally a clear-headed and vigorous thinker, he has attained to a far higher degree of accuracy, both in fact and inference, than is the case with any other electro-therapeutists, unless it be Ziemssen and Duchenne. Space forbids me to illustrate the merits of these volumes by quotation or condensation, nor could I thus do justice to them; I would earnestly recommend them, however, to the consideration of the Sydenham Society.

The first volume deals almost exclusively with electro-therapy as applied to the ear, and is full of interest even to one who, like myself, is comparatively ignorant of ear physic, as it shows how valuable it is to an observer to have to concentrate his energies; and how so minute an inquiry, far from narrowing the mind, may, by the infinite richness of nature, be full of mental nourishment, while, on the other hand, it compels minute accuracy and extreme delicacy in the management of details, and by limiting simplifies the results which elsewhere are bewildering in their multiformity.

By his delicate and precise observations upon the ear Brenner arrived at and perfected his polar method, testing, as he did, the reactions the poles severally obtained from this special sense. What was proved upon it must, of course, be true, essentially speaking, for all parts; and in his second volume Brenner applies the method to the treatment of nervous disease, of cerebral, spinal, and peripheral palsies, and also to the treatment of disease by surgical means. On this latter point it is well known that Dr. Althaus is also an eminent authority, and I hope he will settle for us that most interesting claim of electro-therapy to cure thoracic and other aneurisms. Such operations as electro-puncture in thoracic aneurism, with all the known terrors of embolism before our eyes, is one which I, at least, scarcely feel justified in undertaking until a larger array of experience has been set before us by electro-therapeutists of the skill and experience of Althaus. Meyer's volume appears in an English dress, under the care of Dr. Hammond, the eminent physician of New York, and is published in England by Trübner. Meyer's book is well known, and to write any adequate description of it now would lead us too far, and would lead us also into no little repetitions. Meyer has himself made no slight original contributions to electro-therapy, and the rest of the volume is fairly done, though not in all places up to the mark of the most recent work. It is, moreover, unequal, some chapters being excellent, while others are defective.

Dr. Hammond deserves our hearty thanks for his translation, a boon which is as much for us as for his own countrymen.

The last work on the list is the treatise of Beard and Rockwell, who are already very favorably known as medical electricians. The book has only been in my hands for a few days, so that no careful criticism of its contents is now possible. The impression it gives at first sight is very favorable; it is beautifully printed, and profusely illustrated with woodcuts, in a way which cannot fail to be most useful to students. A woodcut teaches, at a glance, such processes as those of localized electrization, which are hardly learned by long poring over verbal descriptions alone. The matter of the volume, again, is admirably arranged, better than in any book of the kind which I have seen; while, as regards quantities, due space, and no more, is given to those chapters on physic and on instruments which are so often carried to a length not needed by the student of medicine. On turning to what one may call the test-points of the book, it seems in many places to be very well up to the day, and to show no little familiarity with the work of others. References are numerous and accurate, and we are delighted to have that which we lack so much in almost all other such books, namely, complete indexes, both verbal and bibliographical. All this it is easy to see, and it is as easy to see that the authors show some independence of thought and observation, though by no means enough to emancipate them entirely, or in all places, from the imposing falsities of some current authorities. But it is not easy to form at once a judgment upon that part of the book for which the authors could chiefly claim it. They have perfected, if they did not originate, a method of electro-therapy which they call "general faradization," and which they put forward as deserving of careful attention. The account of the process occupies a great part of their volume, and the authors evidently speak from no little experience of its merits. All I can say is that I hope that what they advance is not altogether true, or we have, indeed, a task before us.

The practice of electro-therapy is already burdensome, but what if we have to give sittings of an hour or more to each patient! If our present plans are insufficient, and we find ourselves driven to take up general faradization, we shall have to act as follows. The patient's feet are to be placed upon a broad moist copper plate, which is attached to one conductor, while the other is "promenaded," variously armed, upon the body. The authors strongly advise that this second rheophore should be the operator, he being counselled to move his moistened hand from part to part of the sufferer. They comfort us by saying that after a while we shall rather like the process than otherwise, and, so far from being harmful to us, we shall be galvanized into athletes without any effort of our own. The

patients, if men, must be stripped, if women, must be unlaced so far as to admit of the free movements of the human rheophore. The authors write in no hyperbolical spirit, and they compare general and local faradism in a temperate way, giving the former the preference in debility, disordered nutrition, rheumatism, and sundry obscure chronic diseases. In a word, it is ordered almost in the same cases for which the "electric bath" was formerly recommended; and it is evidently a far more scientific and more promising expedient than the bath, which at a touch of the wand of Duchenne fell into disuse. It is a pleasing feature in the present volume that the unsuccessful cases bear their just proportion to the successful, and that in other instances where success was incomplete there is no attempt to make the results appear more satisfactory than they really were. On the whole, then, Drs. Beard and Rockwell have put forth a well-printed and well-arranged volume, one which seems abreast with the knowledge of the day, and which is written on the whole in a temperate and cautious spirit. It contains, moreover, an account of certain important therapeutical experiments in "general faradization," which command attention and may prove hereafter to be very valuable. One thing in particular is lacking which is lacking in all and several of the books now under discussion, and that is the comparison of electric treatment with the results of treatment by other physical methods. Electricity is spoken of as if it had some subtle identity with neurility, its own mysterious phenomena seeming akin to the mysterious ways of living tissue. Now, in all likelihood neurility is very different from electricity, and is only accompanied by electric disturbances, as are most or all other changes of equilibrium. Between neurility and electricity, pure and simple, however, there are differences so important that we are compelled to regard them as two wholly distinct modes of force. Electricity arouses nerve power, not directly by supplying it as such, but indirectly as heat and other physical forces arouse it. Electricity may be and is probably the most handy and the most efficient of means for improving all kinds of nerve defects, whether motor, sensory or trophic, but no doubt heat and mechanical motion, not to mention other forces, have each their degree of efficiency. It is well known that certain persons have gained great and, no doubt, in some measure legitimate reputations by the methodical application of mechanical motion. Manipulation of individual muscles, flexion, extension and rotation of the limbs, kneading of the viscera, are among the means made use of by such persons, and no doubt with much success. Dr. Althaus very wisely recommends such processes, aided by applications of heat, in the

treatment of infantile paralysis, but he does not seem to have made any wide investigation into such treatment. Gymnastics also form part of the treatment in Queen's Square, but I am not aware that "passive motion" is practised in that admirable institution. Until, however, some one who has the time and the opportunity shall point out to us clearly how much may be done by warmth and by mechanical motion, both active and passive, we shall be unable to say distinctly how large a part of the cures ascribed to electricity are to be ascribed to that means exclusively, or how far electricity takes its place only as one of the many physical means of cure.

T. CLIFFORD ALLBUTT.

V.—The Graphic Study of the Pulse.¹

OF the phenomena of life which the physiologist has to study, there is none more important or more widely distributed in some of its forms than movement. In its simpler manifestations it has always attracted the attention of the student, but it is only of late years that the more complex forms have been the subjects of scientific study. The application of methods of research borrowed from the physical sciences, has enabled the observer to analyse one after the other the features of this phenomenon. The duration, the extent, and the force of movement have all in turn been measured by instrumental contrivances, and the modern school of physiology is now busied in estimating the phases of vital movements, and determining their forms. The graphic method, which yielded such good results when applied by Marey to the study of the movements of the circulation, has now become a recognised means of clinical research. In this new school, which seeks to give medical observation an exactness previously unknown, no one has won a more honourable position than Paul Lorain. Convinced by the early applications of the method that it promised much fruit when applied to the problems of medicine, he has devoted himself to the application of registering-instruments to the study of disease. The volume before us is the second of a series that he proposes to publish. The first, entitled '*Le Choléra, observé à l'Hôpital Saint-Antoine*,' appeared some three years ago, and delighted the disciples of Marey by the excellent illustrations which it gave of the value of the method in the study of acute disease. A novel character

¹ *Etudes de Médecine Clinique, faites avec l'aide de la Méthode Graphique et des appareils enregistreurs.* Par P. LORAIN, Professeur agrégé à la Faculté de Médecine de Paris, Médecin de l'Hôpital Saint-Antoine. "Le Poulx, ses variations et ses formes diverses dans les maladies." Paris, 1870.

was given to each clinical history by the registration of the forms of the pulse and respiration movements, and by charts prepared to show at a glance the whole history of a cholera case. In these charts one line told the story of the temperature, a second and third the variations of the pulse and of breathing, a fourth recorded the changes in the body-weight of the patient, while other curves were arranged with great ingenuity to tell the quantity and modifications of the excretions from day to day. Each chart was a wonderful piece of work, viewed artistically and scientifically; it gave at a glance the materials for a rapid and superficial survey of the progress of the disease, and its effects on each function. All who run may read such diagrams, but this easy reading is not always profitable. The representation of the state of the temperature, or any other condition, by a series of curves which join together the special observations of each day, is, after all, a most artificial method. There is in each curve only so much truth as the individual observations contain; all the rest is at best only an approximation to the truth. We have said this much of some of the necessary defects of the method of which Lorain is so able an exponent, because we feel that there is some danger in the tendency to popularise the graphic study of disease. In the striking results produced, men are apt to see more truth than there really is. Having said this, we can heartily welcome the second instalment of Lorain's labours. The objections we have hinted to the elaborate charts of his former work, are by no means applicable to the sphygmographic records by which the present volume is enriched. Whatever may be the fate of the method in other investigations, we are sure that in the study of the movements of the circulation and respiration it is destined to play a great part in the advancement of medicine. The study of the variations of the form and rhythm of the pulse in disease, has afforded our author an opportunity of proving the high scientific value of the results collected by registering-instruments. He divides his matter as follows:—1. An exposition of the method. 2. An analysis of the works of authors who have treated the same subject. 3. The practice of sphygmography. 4. Clinical researches on the form and rhythm of the pulse in disease; and, lastly, therapeutical and medico-legal investigations made with the sphygmograph. The first two sections are comprised in a single chapter, which commences with some introductory observations on the use of Marey's instrument. The greater part of the chapter is however, occupied by a most carefully compiled analysis of the views of other authors. After Marey, come the Germans, whose works supply most of the contents of the chapter. The mechanical and physiological part of the question has already

been so fully treated in these pages, and so little has been added since it occupied us, that we shall not now go over the old ground. We can, however, heartily recommend this part of the book to all who have not had access to the original monographs, and who want a readable account of the painstaking labours of the German school. There is, we notice, scarcely justice done to those who have written on the sphygmograph in this country, and the references in several places betray an ignorance of the extent of the work of the English observers. The chapter, too, would be more satisfactory if it contained some expression of the views of Lorain himself. It is somewhat disappointing to wade through some hundred pages of compilation, and then to find that the author offers no guidance whatever in the selection of the theories broached. In a purely practical record of clinical work, such as this professes to be, it was, of course, not absolutely necessary to enter on the physiological part of the subject; but to the student the book would certainly have been increased in value by the expression of the views of one so competent as the author to decide where doctors disagree.

The second chapter, on the mode of using the sphygmograph, covers ground which has been specially worked in this country. So many modifications in the shape and mode of applying the instrument have been suggested, that no doubt many have been deterred from attempting its use. Our author dispenses with all these modifications, and uses the sphygmograph in its original form. His experience in teaching others to apply it has led him to enumerate the following common obstacles to its accurate use. The first and greatest is hurried application. We heartily concur in this, and quite agree that no observations require more patience or more skill. The shape and development of the forearm, the position of the radial artery, its deep or superficial site, its size, direction, and relation to the tendons, all modify the ease of application. The tightness of the band or bands which bind the instrument to the arm, and the regulation of the pressure of the spring on the artery, are all points which the author thinks cannot be taught by rule, but must be acquired by practice. He, however, admits that the instrument does not mould itself equally well to all arms, and this admission, we think, is a strong justification of the English practice of removing the side wings of the sphygmograph, and fixing it on the forearm by elastic bands instead of by a lace. Our own experience certainly leads us to prefer the use of a pad, on which the arm can rest and to which the band may be attached. After all these preliminary difficulties have been overcome, there still remains the great difficulty of obtaining what Lorain well calls the maximum rendering of the pulse. To do this, the pressure of the

spring on the artery must be most carefully adjusted, for the trace may be completely spoiled by bad management. But even with the most skilful application, it requires great patience to obtain the best trace of the pulse; for as he justly says there are—"beaucoup des tracés qui sont bons, mais qu'il n'y en a qu'un qui soit le bon." This adjustment of the pressure on the artery Lorain leaves to the skill of the observer; and though we are willing to admit with him that ill-judged attempts have been made to seek indications in the sphygmographic trace concerning arterial tension, which the instrument is not well adapted to afford, yet we feel bound to say that some method of measuring the pressure on the artery must be adopted before any comparison of pulse traces for the purposes of generalization can be attempted. Indeed, elsewhere the author somewhat inconsistently admits this, when he expresses the opinion that all sphygmographs should be regulated before use, as is done for other scientific instruments employed in collecting observations for comparison. We were rather surprised to learn that Lorain still prefers the use of smoked paper to smoked glass for collecting the trace. The glass certainly offers many advantages in the diminished friction and the ease of reproduction of the trace by photography. On the choice of an instrument there are some words of value. It should be of the full size, armed with a flexible lever sufficiently long to amplify the trace; the clock-work should move the plate rapidly rather than slowly, because with a rapid transit the descending part of the trace is recorded, with its special features more clearly developed. Finally, the student is warned that all observations which are to be used for comparison should be obtained by the same instrument. A large number of traces are given which show in a striking manner the modifications in form which may be produced by the rate of transit and the pressure of the spring. These effects, it is true, have been long known to all who are practically acquainted with the sphygmograph, but we have nowhere seen them so well illustrated.

The influence of emotion, exercise, pain, &c., in producing variations in the size and shape of the pulse trace, are next carefully described, and then we come to some very interesting remarks on the effects of the position of the arm in increasing the amplitude of the trace. The elevation of the arm has long been a familiar mode of demonstrating at the bed-side the visible bounding pulse of aortic insufficiency; applying this knowledge to sphygmographic observation, M. R. Lépine has made some interesting researches on the comparative development of the trace collected with the arm held horizontally and vertically. Lorain, to whom these researches were commu-

nicated, has made a number of observations of his own, which show how greatly the pulse trace may be amplified by this simple expedient. The theory of the fact is very simple; when the arm is elevated the radial artery during the interval between each cardiac contraction is empty or almost empty of blood, and consequently each systolic wave produces a movement of the arterial walls, which is the greater the more completely the artery is empty. We can confirm the remarks on the value of this plan in studying some forms of heart disease. It often happens that in mitral regurgitation the small, flickering, radial pulse gives a trace so mean and miserable in all its features as to be worthless for purposes of analysis. Let the observer in such cases raise the arm to a vertical position, and a trace will be recorded many times more ample, and presenting all the secondary waves in the line of descent. By the same simple procedure all the peculiarities of the arterial movement can be collected in those cases of extremely rapid heart action which, in the horizontal position of the arm, defy the most skilful investigator.

The pulse of convalescence from acute disease has long been recognised as possessing two qualities, feebleness and slowness. The sphygmograph analyses these qualities still further, and shows that there are three great characters—slowness, irregularity, and polycrotism, a term suggested by Vivenot, to signify the development of a number of secondary waves in the line of descent. The feebleness of the pulse may be associated with considerable amplitude of the trace and polycrotism, or in very debilitated subjects the trace may be very small and have little or no development of the secondary waves. The irregularity of the pulse during convalescence seems to be more common in the old than in the young. The febrile pulse has two main characteristics—frequency and dicrotism. The dicrotism has been so much misunderstood that some remarks are devoted to its explanation, but no suggestion is offered as to the mechanism of its production. The *pulsus bis feriens* is accepted as the synonym, but the reader is warned against the mischievous notion that dicrotism is a morbid phenomenon. It always exists in a greater or less degree, but it is only when it is carried to its extreme limits, that is to say, when the second pulse wave is equal in size to half the first, that the *pulsus bis feriens* becomes apparent to the finger, and the febrile form of dicrotism is reached. The varieties of dicrotism are, according to our author, the median or symmetrical and the unsymmetrical; the first term is used when the dicrotic wave is placed midway between the end of one and the beginning of another cardiac systole; when it is too near the end of the systole, it is called unsymmetrical. This chapter contains a good deal of use-

ful matter, but there is not much that is new; the most interesting traces, perhaps, are those taken in the different stages of intermittent fever. The slow steady pulse of moderate tension is shown to change, as the shivering comes on, to the small irregular pulse of high tension, somewhat like that of cholera; and then, as the reaction follows, the pulse steadily grows in amplitude till it assumes the full dicrotic form. There are also a number of illustrations of the pulse in pneumonia, pleurisy, asthma, cholera, &c. In the last-named disease Lorain shows clearly that the pulse is often a very untrustworthy guide, and may afford no signs of reaction long after the temperature has indicated it, and *vice versâ*. The temperature and the pulse in cholera do not obey the same influences, and the former is often the more trustworthy witness. Of all febrile pulses, that of acute rheumatism is the most striking, on account of its great amplitude, pointed summit, and full dicrotism. The low state of arterial tension associated with the copious sweats, is generally held to account for these features, but there seems to be in addition an increased vigour of the heart's action, for in purpura hæmorrhagica, with equally low tension, there are all the features of the rheumatic pulse except those of increased cardiac vigour. The supervention of pericarditis occasionally shows itself by an increased tension, and we think Lorain errs when he says that in the immense majority of cases the sphygmograph gives no sign of its occurrence. Rheumatic meningitis, on the other hand, he thinks is indicated by irregularity in the pulse trace. The slow pulse of icterus is well figured, with its increase of summit, preceded by a hook-like point. Compression of the abdominal aorta increases all these peculiarities of the trace, which presents a striking resemblance to that of puerperal women. Lorain refers these features to the fact that in hepatitis and jaundice the liver circulation is impeded, and hence the signs of increased arterial tension.

The curious spirit of French investigation is well illustrated by the chapter on the pulse of lying-in women. Traces are given which we think no English observer would ever have collected. The pulse form before delivery, after delivery, during the pains, during the passage of the child, and immediately after its birth, are all recorded. The value of these is, we think, much diminished by the modifications produced on the traces by the muscular movements, rigors, and respiratory efforts of the women. The changes in the pulse which follow delivery are, however, very instructive. There is at first a steady increase of amplitude and diminished frequency; on the third day the tension rises still more, and about the fourth there are signs of febrile reaction. If any inflammatory complications threaten,

the slow pulse of high tension is replaced by a rapid dicrotic pulse purely febrile in its form. The occurrence of this febrile pulse does not, however, always indicate a grave disorder. Lorain shows that it sometimes depends on slight mammary troubles.

When Marey published his celebrated work, one of the most remarkable facts announced in it was the form of pulse trace peculiar to lead poisoning. This trace was characterised by amplitude, large rounded summit, high position of the dicrotic wave, and the development of other secondary waves in the line of descent. Marey's observation was soon verified, and the pulse trace was recognised as one of high tension, somewhat similar to that of atheromatous vessels, and in some respects like that produced by compression of the abdominal aorta. Lorain gives numerous and very faithful illustrations of this pulse, but ventures to express no opinion as to its mode of production. It has often occurred to us that the trace bears a striking resemblance to that of cases of granular kidney, in which there is a high state of arterial tension depending on hypertrophied heart and capillaries. As this pulse form is not constant in lead poisoning, but in our experience is often associated with albuminuria, and occurs mostly in old cases, it is worthy of future inquiry to determine its connection with the kidney lesions which lead is so apt to produce in certain constitutions. The peculiar muscular tremors which lead in common with other metallic poisons causes, are well marked in some of Lorain's traces, and the effects of that ataxy of the heart and lungs which sometimes occurs are also well figured. The difficulty of making observations on persons affected with constant muscular movements would have discouraged any ordinary investigator; but Lorain has, like an earnest student, conquered all such obstacles and even converted them to good account. With a most praiseworthy patience he has accomplished the task of classifying these apparently irreducible irregularities, and as the fruit of his labour he now shows us that the tremors of lead, mercury, alcohol, &c., as well as those occurring in such nervous affections as paralysis agitans, have each their special characters. The results figured in these few pages, which illustrate the use of the sphygmograph as a myograph, are evidence of the truly scientific spirit in which our author has done his work, and if he had taught us nothing else, he deserves for these observations alone no small share of earnest gratitude.

The chapter on heart diseases opens with some very good remarks on the position of the sphygmograph as an auxiliary to the older methods of diagnosis, and a warning against the mistake of elevating it to the dignity of an independent guide.

There then follows an admirable collection of tracings from each form of valvular disease, with commentaries on the influences which may modify the typical characters. The trace of aortic insufficiency occupies, as might be expected, a large share of attention, and the errors of the earlier observers in attaching undue importance to the hook-like summit are pointed out. The elevation of the arm is shown to very often lose its amplifying influence in this form of disease, a fact which we think may be accounted for by the pressure of the spring on the artery. The pressure, which is adapted to collect the maximum trace in the horizontal position, becomes too strong for the still lower state of tension induced by the vertical posture, and consequently the trace is diminished in size, and its finer features are lost or obscured. There is one fact in connection with the diagnostic value of the sphygmograph in aortic disease to which we think justice has scarcely been done. It is the value of the trace in indicating the presence of any obstructive disease at the aortic orifice in cases of well-marked insufficiency of the valves. The two conditions are frequently combined, but the systolic murmur, which is generally supposed to depend on obstruction, is in our experience frequently present when no such lesion exists. The double aortic murmur by no means always justifies the diagnosis of double aortic disease. In these cases, in which the ear fails us as a diagnostic guide, the sphygmograph comes to our aid, and very constantly indicates the presence of obstruction by a prolongation of the summit of the trace. In the absence of such lesion the first secondary waves are well defined, and not lost in the prolonged summit. In many cases we have verified this by post-mortem examination, and although the faulty application of the instrument may lengthen the top of the pulse curve, and a similar condition is present in advanced atheroma, we nevertheless regard this as an important diagnostic use of the sphygmograph. Of pure aortic obstructive disease there are some excellent traces, which fully confirm the truth of the form indicated as characteristic by Marey and his earlier followers. One case is given in which digitalis was prescribed for a case of this kind. The effects were a remarkable exemplification of the slowing action of the drug upon the heart. The pulse, beating ordinarily at 34 per minute, fell to 27 and 26, which is not far from the same proportion as a fall from 70 to 58. This case well shows that a slow pulse may still feel the effects of digitalis, and be so much reduced as to cause alarming symptoms. The traces devoted to the subject of mitral valve lesions do not exhibit any new facts. The irregularity of the pulse in both forms of mitral disease is rather too much dwelt on. The irregularity is so

generally recognised that it would have been well to print examples of that regular pulse which is by no means infrequent both in regurgitant and obstructive affections of the orifice. The classification of the irregularities of the pulse in all cases would have been a subject worthy of Lorain's powers of analysis. There can be no doubt that the very striking forms of disordered rhythm in valvular diseases obey some law. Marey hinted at the explanation, but at present no precise ideas have been advanced. The respiratory functions have been shown to be so intimately connected with the variations in arterial tension that the solution of the problem will most probably be found in this direction. There are some observations which seem to tell that Lorain has not overlooked the question, and we trust we may soon have his views before us. It is most noteworthy that on aneurisms very little is said, the traces from two cases only are published, and in neither does there appear to have been any autopsy. In aneurism the sphygmograph may effect so much in teaching the precise seat and relations of the sac, that in every case it is most desirable to have full anatomical reports to compare with the pulse modifications. From the little that has already been done in this direction, it would appear that we only want a good series of such reports to have in the sphygmograph an almost infallible guide in the diagnosis of the most obscure cases. In addition to the author's two cases, others published by Bayol, Brongrest, Foster, Seguy, and others, are referred to. Following the remarks on aneurisms, there is a most instructive abstract of a paper by M. Potain on the movements and bruits which occur in the jugular veins. Potain, by a very ingenious application of the cardiograph, collected synchronously the traces of the venous movements, and those of the heart's impulse, and of the radial and carotid arteries. In a single plate these four movements are recorded, vertically one above the other, and the double elevation of the trace of the neck veins is shown to correspond with the contractions of the auricle and ventricle, while the falls which depend on the emptying of the jugulars are shown to correspond with the diastoles of the auricle and ventricle respectively. These facts conclusively prove that the venous movements are totally independent of those of the neighbouring arteries. With similar ingenuity the venous bruits are shown to occur at the time the vein empties itself to fill the dilating auricles, and consequently not to be due to a reflux of the blood.

The last chapter is devoted to the therapeutics of heart diseases, and contains some remarks of value on general treatment. The author thinks hygienic measures are too much neglected, and says that when an orifice is altered by disease

and associated with more or less hypertrophy, everything which tends to diminish resistance and augment the force of the heart, should be regarded as a means of relative cure. To augment the heart's power, a dietary from which are excluded all substances which promote fatty degeneration should be observed; regular exercise, to promote the activity of the circulation without distressing the heart, and a careful regard for all conditions favorable to the lung functions, are the other indications. To diminish the resistance, we must lessen the weight of the patient, for as the weight of the blood is in direct proportion to the body weight, the diminution of the latter lessens the weight which the heart has to move. In this way diuretics and purgatives act with advantage, but the direct abstraction of blood interferes too much with nutrition to be generally safe. The great *impedimentum*, however, is fat, and this is only to be removed by a careful dietary. By such means, our author justly observes, life may be often prolonged, and all the accidents of valvular lesions considerably postponed. The diminution of the weight of the patient, and the consequent diminution of the mass of the blood, relieves the heart from the struggle with charges of blood which are too heavy, and places the orifices which are altered in a more favorable relation with the blood which has to traverse them. At the same time the hypertrophy will cease to increase, and will probably diminish; for it must not be forgotten that this hypertrophy is only a mode of accommodation comparable to that of the uterus during gestation, and similar to that of the bladder and stomach when there is any obstacle in the direction of their outlets.

Such is the substance of the remarks on general treatment, which, if they possess little novelty, are at all events dictated by sound sense. In treating of the action of digitalis on the heart, our author has more definite information to give us. An analysis of the investigations of Legroux, Bordier, Constantin Paul, and Hirtz, precedes his own work. The fifty traces which are given show unmistakeably that in moderate doses digitalis increases the force of the heart, while it slows and regulates its action. In large doses ataxy of the heart is produced. The small irregular pulse of mitral insufficiency is given as the first example, and in successive traces are figured the increased amplitude of the pulsations, the gradual slowing of the pulse, and the restoration of regularity, all of which the remedy effects. The slowing of the pulse when the administration is long continued may be so decided, that a fall from 70 to 32 is recorded in the first series of illustrations.

This excessive slowness has long been known, but, as the author points out, it is more apparent than real, for it is not

fixed, but on the slightest perturbing cause may be succeeded by a very frequent pulse. On auscultation, moreover, Lorain says twice as many cardiac contractions can be heard as the pulsations at the wrist indicate. In many pulse traces these small abortive pulsations are to be recognised by a slight elevation in the line of descent. The regularity as well as the slowness is easily disturbed; it will often suffice to make the patient walk across a room to bring back all the typical irregularities of mitral regurgitation. These facts, Lorain thinks, explain the diversity of opinion which has prevailed with reference to such purely objective phenomena as the effects of digitalis on the frequency and regularity of the pulse. Registering apparatus, he thinks, must finally decide questions of this kind; they must no longer be matters of opinion or authority, but the irrefutable evidence of the instrumental record must be advanced.

Digitalis is precisely the remedy to engender differences of opinion, acting as it does very variously, and hence we cannot be surprised if in the absence of scientific methods of observation the results have been contradictory. The unstable character of the pulse produced by digitalis is illustrated by a series of tracings taken from one case of mitral insufficiency, in which a slow and regular pulse was first produced, then greater slowness, followed by irregularity, and then slowness and regularity again. When irregularity is produced it is of two kinds, 1. Simple irregularity or inequality of the intervals, with acceleration of the pulse; and, 2. Rhythmical irregularity, or the rhythm of twin pulsations, two pulsations close together, the second followed by a long pause. These peculiarities are so characteristic of digitalis poisoning, that Lorain in one instance diagnosed poisoning by digitalis from the study of the pulse traces alone, in the case of a woman, in which inquiry proved the truth of his surmise. Most of these effects have been noticed by previous observers, especially by Traube, but the confirmation of them by one who has relied on instruments of precision for all his results is nevertheless valuable. A study of all the traces shows that the two most constant effects of the remedy were increased amplitude of the trace, and slowing of the pulse. The arterial tension was also evidently increased. In some cases in which the poisonous effects were produced cardiographic traces well display the complete ataxy of cardiac movement.

Such is the work of Marey's chief follower in France. It is a careful record of nearly 500 observations, made with no ordinary skill by one who has conscientiously avoided the seducing influence of theory. This is its great merit. In some

respects the book might have been made more useful to the student, and the addition of a little speculation would have rendered it pleasanter reading. The author's object, however, has been to give a faithful account of his experience of the sphygmograph as an instrument of clinical research; and in doing so he has sought less to discover new facts than to place our existing knowledge on a sure basis.

As a consequence, there is more work than thought in the book; but it is nevertheless more valuable than many that are more original, and the most complete and trustworthy text book of the sphygmograph which has yet appeared.

B. W. F.

VI.—Laycock on Mind and Brain.¹

It is eminently difficult to review a work of Dr. Laycock's in the pages of a periodical to which, as is the case with the 'Medico-Chirurgical Review,'² he has himself contributed so much valuable matter. If we venture to differ from our author, we shall be suspected of ingratitude for his past favours; if we bestow even a scanty meed of praise upon him, we shall be suspected of being actuated by a lively anticipation of future contributions to our Journal. With a Scylla thus on the one side and a Charybdis on the other the present reviewer has prudently determined to take a line as nearly as possible equidistant from both, and to lay before his readers, firstly, a bare skeleton outline of the main divisions of Dr. Laycock's book; and, secondly, to enumerate, with a very short exposition, the various doubtful questions of wide bearing, either as to method or as to principle, upon which the author pronounces definitely, but as to which it is competent to other thinkers to demur to his conclusions.

Dr. Laycock's work now before us is a second edition of the work on 'Mind and Brain,' published under the same title eleven years ago, the principal addition to the former issue which the present edition contains being to be found in the last chapter (chap. xiii, "Fundamental Laws of Vital Action") of

¹ *Mind and Brain, or the Correlations of Consciousness and Organization, systematically investigated and applied to Philosophy, Mental Science, and Practice.* By THOMAS LAYCOCK, M.D., &c. &c., Professor of the Practice of Medicine and of Clinical Medicine, and Lecturer on Medical Psychology and Mental Diseases in the University of Edinburgh, &c. &c. With a Preliminary Dissertation on the Blood, and Illustrations of the Text. Second Edition. 1869.

² See 'British and Foreign Medical Review,' vol. xix, Jan., 1845, pp. 298—311, "On the Reflex Action of the Brain;" July, 1855, pp. 155—187, "Further Researches into the Functions of the Brain."

the first volume, and the principal alteration in the other direction being the omission, from the end of the second volume, of some sixteen pages of an appendix on "Examination of Dr. Carpenter's Claim of Priority as to the Discovery of the Law of Unconscious Cerebral Action." His second edition is, in many of its most important chapters, an identical reproduction of the first; page 184, for example, is Volume I, and in a chapter on "Latent Consciousness," a subject upon which Dr. Laycock has in our columns and elsewhere bestowed much labour, stands in the second edition with the note referring to the now abolished appendix unremoved, as though that historical reclamation still remained as an *aculeus in fine*.

Each of the two volumes now before us treats of three distinct principal subjects. The first volume comprises, firstly, a dissertation on method, extending over a little less than a hundred and twenty pages, and constituting, perhaps, the most valuable of the more purely metaphysical portions of the work; secondly, it presents us with a hundred pages of "metaphysics," under which term ontological as well as psychological disquisitions are comprised; and it is completed by a third factor, equal in bulk to both the other portions of the volume taken together, in which an attempt is made to correlate the laws of life and those of consciousness upon teleological, which are more or less indistinguishable, in several aspects, and in several parts of this division of the work, from purely theological principles. This third part of the first volume bears a title which, offering us an alternative, explains its scope and argument. This title is "Mental Dynamics, or Teleology."

The second volume treats, in three divisions, of the Principles of a Scientific Psychology; of the Principles of a Mental Physiology; and of the Principles of Mental Organology. The two latter divisions, verging, as will have been anticipated, more directly into pure biology than the other parts of the book, and enabling us, besides, to raise ourselves here and there into elevated stand-points, from which glimpses may be obtained through clefts in the mist below our feet into the fertile, though only partially enclosed and cultivated lowlands of pathology and therapeutics.

Having thus specified the great divisions into which this work separates itself, we will now describe the position into which Dr. Laycock avows that he puts himself, and into which, therefore, his readers must put themselves also if they are to travel along the same road with him in peace and amity.

"The student of mental science," says Dr. Laycock (vol. i, p. 4), "if he would be a successful student, must concentrate his researches upon the laws of action of the brain and nervous system, as they

correlate the laws of thought and volition, and eschew all efforts to develop it by simple meditation on his own states of consciousness, or by the processes of the logical act to the exclusion of observation and experimental research on the subject-matter of his science."

And "the brain and nervous system," we are told on the preceding page, "are the proper subject-matter of a true science of mind." We do not admire the practice so familiar to the word-catching dialectician of citing a number of passages from an author's works which are not in coherent contexts, and are not, as may be imagined, always coherent or consistent with each other; or we might dwell upon the fact that Dr. Laycock, like any one else, can be found confessing (see p. v, preface) that we "have no other sources of knowledge than those which our consciousness affords;" and we will content ourselves with observing that by the apposition of two such quotations, and the garnishing of them with a few words of corollary, any dexterous gladiator of the class we have alluded to has it always in his power to win himself a victory. But we would submit that, whatever we may suppose, conjecture, or suggest, it is a fact that we *know* absolutely nothing of the way in which the laws of action of the brain and nervous system "correlate," or have subordinated to them the laws of thought and volition; and, more, that we cannot conceive even, how any physical process, whether in the way of growth or in that of any less complex process, can *connect itself* with states of consciousness. It is a fact, but at present an ultimate fact, that the mind, and the body especially, but not exclusively, in its grey vesicular nervine constituents, are connected; it is also a fact, but still an ultimate fact, that they act and react each upon the other, mental changes and mental labours acting in our daily experience upon our material organisms and material substances, in their turn changing the current and altering the state of our thought, as in opium, chloroform, alcohol, or gout-poisoning! but the recognition of these facts, as facts, is a very different matter from discovering how this mutual interaction is effected; and it is not at all clear to the present writer that we are very much nearer to a *scientific* solution of the question than we were in the days of Leibnitz, who solved the riddle, a *dignus vindice nodus*, it may be confessed, by the super-scientific hypothesis of a pre-established harmony, which Sir William Hamilton did not trouble himself to combat or refute (see 'Lectures on Metaphysics,' vol. i, p. 304), leaving the task to Mr. Herbert Spencer, whose plan of dealing with it may be found at pp. 419, 420 of his recently-published first volume of 'Principles of Psychology.' Allotropic transformation is one thing; chemical change and unstable, because highly complex, compounds is another;

growth of living protoplasm, which entails the occurrence of this second set of changes, is yet something more complex than they, and is, therefore, a third thing; mental processes are a fourth, connected, it is true, invariably, or all but invariably, within our experience with the third, but in a manner which science as yet knows not how to investigate, and which few scientific men have any hope of discovering. "Physiology," says Mr. Herbert Spencer (op. cit., p. 48), "ceases to be physiology when it imports into its interpretations any psychical factor, a factor which no physical research whatever can disclose, or identify, or get the remotest glimpse of."

It would be unfair to Dr. Laycock not to put prominently forward the fact that the entire latter half of his first volume is one long and complex antidote to the corollary which can be drawn from the two last lines of page 1 of that volume, where we read that "what the man is in character and conduct is the expression of the functions of his nervous system." Now the general bearing of this, the third, portion of Dr. Laycock's work may be briefly summed up in the following propositions, to the forming of which we must confess that our author's own words as contained, not in his own book which we are at present engaged upon, but in a paper of his in our *Journal* of July, 1855, have helped us so considerably as to make us think, when face to face with his obscurer utterances as Professor in Edinburgh, that a removal thither from York may have acted upon him much as a sojourn in Laputa acted upon the plain inhabitants of Lagado. Mind, we understand Dr. Laycock to consider to be the ultimate (he sometimes calls it, and most inconveniently, the "final," pp. 15, 226, vol. ii) cause, the ordinary force of all the physical forces; and "mind" he holds to be an emanation from the great creative intelligence, and as manifesting itself to us in an orderly design detectable throughout the 'Kosmos'; he considers it to be a manifestation to us of the divine volition. The working of mind being thus established upon teleological considerations, Dr. Laycock proceeds to define this working by the employment of language which many different schools of metaphysicians could indifferently avail themselves of. He teaches, as indeed (see p. 144, vol. ii) he taught "more than twenty (thirty?) years ago," and as Aristotle, Stahl, and the younger Fichte have also taught, that the processes of life which never rise into the horizon illumined by consciousness, no less than the most purely intellectual and spiritual processes, are all alike to be ascribed to the diverse operations of one and the same principle, viz. the "mind," of which the title of the third part of his book speaks. Consciousness is attained to by the individualisation of this universal mind in the

human cerebrum, which then becomes cognizant of its own working, and derives its ideas from the changes which it then itself brings about.

Our first objection to this scheme as a whole is that it is, in one word, reducible to pantheism; or, in other words, that the consideration of "mind evolved as teleiotic ideas," to use Dr. Laycock's language, throughout inorganic and organic creation, or to use Wordsworth's poetical expression of the same set of thoughts, that—

"the sense sublime
Of something . . . deeply interfused,
Whose dwelling is the light of setting suns,
And the round ocean, and the living air,
And the blue sky, and in the mind of man,"

does not, apart from other arguments, furnish a sufficient, nor indeed any basis for the doctrine of personal identity, and various other doctrines which depend upon it. The younger Fichte and his school, indeed, whilst agreeing with Dr. Laycock in holding that it is only in virtue of being brought under organic condition that the soul is within our experience raised into the region of consciousness, would, if we mistake not, say that he had exactly pursued the relation of the two doctrines in question, and that all such augmentations must start from, and not terminate in, that of the peculiar original distinctive personality of the human soul. It is a dangerous boldness which—

"Takes the *a priori* road,
And reasons downwards."

Secondly, we submit that it is competent to any one to demur to the employment of final causes as a guide in an investigation in which professedly (see p. xiii, preface) "the phenomena are examined wholly apart from those philosophical and theological speculations which are altogether foreign to science." Indeed, as it appears to us, final causes are to be looked upon rather as a goal to which than as a clue by which the human philosopher should guide his steps and direct his eyes. Dr. Laycock (vol. i, p. 107) follows Mr. Mill, who in his turn followed Aristotle, in explaining teleology by the help of the analogy which the arts of life furnish us with; and though many metaphysicians would object to considering such an argument to be anything but a paralogism, we should not be prepared to ascribe any inherently self-contradictory characters to the aphorism, "Nature is the Art of God." But neither, again, should we feel quite sure of our ground in taking, with Whewell, "the idea of a final cause as a fundamental and regulative idea to our speculations concerning organized creatures," at least if we are to include investigation under speculations, and if a regulative

idea is to direct and guide us further than by inspiring us with caution. Faith is of things hoped for, science is of things seen, and seen clearly; the knowledge of final causes may be the delectation of the future, the observation of sequences and co-existences is the business of the present. We may hold that—

“All nature is but art, *not known to thee*,
All chance, direction, *which thou canst not see.*”

But the last words, which we have italicised in each of these lines, show that their writer, or rather their writers, recognised the distinction we would here draw.

From what has already been said, it may be anticipated that we shall also demur to the legitimacy of the method which Dr. Laycock (vol. ii, p. 184) recommends for “examining into the structures of organisms,” and which, “fixing definitely upon a primary teleioteic idea or law of development, examines them not only *per se*, but as constructed according to that law in relation to each other as part of a harmonious whole, and in relation to their functions as the organs or instruments of that whole. Such a method of determining homologies, based as regards the mere development of organisms and their leading structures upon an archetypal idea, will go beyond that gross morphology to which it has been hitherto too much restricted; and by tracing the fundamental teleioteic ideas contained in the archetypal idea through their subordinate manifestations in structure, and though the various processes of differentiation and integration to which they are subject attain to a knowledge of the complex organs and tissues not attainable by any known means of research. In like manner as to the function of organs constructed according to a teleioteic idea, they energise according to the same idea. Hence, a classification of ideas (that is, of ends aimed at and attained) will correspond to classification of functions based on a teleioteic anatomy of the parts which make up the organism; or, in other words, both structure and function must be classed according to an arrangement of teleioteic ideas.”

The method here recommended is, as it appears to us, very closely similar to the so-called “Historic Method,” by the employment of which for the resolution of problems of another kind we were recently informed from the pulpit by a certain well-known preacher that “the now all but extinct school of Tübingen” has obtained all its results and triumphs. According to the divine in question, the method which he referred to to with not a little sympathetic laudation consisted in judging of the men, events, and opinions of any previous period, not as though they were isolated individual objects of study, but as

they could be lighted up by the knowledge we had of the age to which they belonged. And he proceeded to affirm that it was by similarly judging from what we knew of the climatic and other general conditions of any geological period that we gained our *knowledge* of the habits of any creature which we might discover fossilized in the rocks of such period. The general conditions which surrounded us at the time in which these remarks were uttered prevented us from entering our protest against their general tenor then; in the pages of the 'Medico-Chirurgical Review' we may make bold to remark that the "Historical Method" to which Dr. Laycock's Anatomical Method bears a very strong likeness, really amounts to judging of the nature and properties of parts by means of some or any imaginative conception which we may choose to project before us of the whole which they and the other factors make up. The present writer can pretend to very little more knowledge of the theological school of Tübingen than that which he gathered from the discourse in question; but if it is "all but extinct," he has to say that its extinction appears to him to have been well deserved, and that he hopes that its history may serve as a warning to other schools in other subjects against the employment of a "historic method," except the "natural historic" method, and indeed against the employment of any single argument which can be reduced to an attempt to explain *obscurum per obscurius*. The physiological philosopher will find nothing to object to in Dr. Laycock's remarks on the formation of those "traces" by which memory is rendered possible; the metaphysician, we take this opportunity of remarking, if metaphysician there be who is ignorant of German, can read to his heart's content of these "traces" as expounded by Dr. Raue, a Professor in the Medical College of Philadelphia, in a purely metaphysical sense, in his "Elements of Psychology on the principles of Beneke;" that work having just been translated into English, and containing a clear statement at pages 9 and 268 of "traces" as embodied both in soul and in body substance. We take the opportunity of repeating here from Dr. Laycock's paper in the 'Medico-Chirurgical Review' for January, 1845, p. 310, Haller's views upon this formation of "traces:"—"Eas mutationes" (says this thinker) "in sensorio conservitas *ideas* multi, nos *vestigia* (Spuren, Beneke) verum vocabimus quæ *non in mente* sed in ipso corpore et in medulla quidem cerebri ineffabili modo incredibiliter *minutis notis* et copia infinita inscriptæ sunt."

Dr. Laycock, in the two latter portions of this work, leaves the somewhat airy and speculative regions into which we have been attempting to follow him, and comes down to deal with

more purely physiological and, occasionally, even with nosological problems. His name will be connected in the minds of most of our readers even more intimately with the doctrine and practice of diagnosis for physiognomy and temperament than with the doctrine of the reflex action of the brain, his claims upon which, we may remark, were handsomely acknowledged by Dr. Carpenter, in a report of a lecture delivered before the Royal Institution, March 1, 1868. On this account we think it well to enter a protest against any mixing up (see vol. ii, p. 310) of the valuable results of pathological observations to which we allude, with any speculative views based upon more or less correct accounts of the stratification and differentiation of the layers of the early embryo. Dr. Laycock (l. c.) says, when speaking of the five various temperaments, the arthritic, the vascular arthritic, the rheumatic, the lymphatic and strumous, and the nervous, which his observation has enabled him to identify in his patients :

“ There are, however, modifications of these temperaments of a general and fundamental character, which depend upon differences in the mode in which fundamental tissues are evolved, and the vital energies proper to them manifested, and which require notice here. The germinal layer of the embryo is composed of two fundamental or primordial tissues—the so-called mucous and serous layers—out of which the nutrient and locomotive apparatuses are respectively developed. Now, between and from these, the blood and the vascular layer of the embryo are formed; and finally the cerebro-spinal centres, destined to co-ordinate, all make their appearance. There are corresponding temperaments developed.”

After this there follow detailed descriptions of the five temperaments above specified, each alike vitiated, as we think, more or less by this retrospect to embryonic morphology, even when we disregard the fact that the division of the germinal layers which Dr. Laycock adopts has been now for a long while superseded, and made antiquated by the discoveries of Remak. When it is a question of what Virchow would call the “histological substitution” of one tissue for another, such as the substitution of bony for cartilaginous, fibrous, or tendinous tissue, or when it is a question of the ways and means and points of origin for the replacement or regeneration of damaged or destroyed tissues, then we believe the facts of the case may be conveniently expressed and colligated by language referring to the three primitive layers of the embryo, the so-called sensory layer; the “motor germination;” and the “trophic” layer, whence the glandular and other epithelial elements of the digestive tract, from the stomach onwards, are evolved.

But in more complex matters, such as the rationale of dia-

theses, the "unifying function of the blood-cells," of which Dr. Laycock himself speaks (vol. ii, p. 473), obliterates all morphological distinctions, and brings all organs, whether glandular, motor, or sensory, into an inextricably interdependent relation of complementary nutrition. Let us test these perhaps ponderously worded principles by an appeal to facts within the very possible experience of every practitioner of surgery or medicine. The cerebral hypertrophy of rickety and, indeed, of children who cannot be spoken of as being otherwise rickety than in respect of the particular affection we are about to speak of, is well known (see Rokitsansky, "Pathological Anatomy," 'Sydenham Society's Translation,' vol. iii, p. 376) to depend upon an excessive accumulation, not of the differentiated neurine, but upon the intervening, connecting, and supporting reticulation of "neuroglia" (Virchow), itself a dependency of, and a continuation inwards, of the pia mater. It is possible, we know, for this tissue, when thus hypertrophied, to remain as a simple lymphoid, interstitial, adventitious growth, and the large head produced by its exuberant increase justifies the Scotch proverb, "Muckle brain, mickle wit." But it is also possible, as Henle and Merkel think (see 'Zeitschrift für Ration. Medicin,' B. 34, p. 49, 'Zeitschrift für Wiss.-Zoologie,' Bd. xx, Stida), and they are somewhat weighty authorities, that this indifferent tissue may develope into true nervous substance, and that the large head may thus come, under circumstances favouring this upward evolution, to be filled with a large brain of really fine quality. But it will be obvious to all men that Henle and Merkel, before suggesting this possibility, must have convinced themselves that Bischoff is right in holding that the two inner membranes of the brain are differentiations of the primitive medullary plate, out of which the cerebro-spinal centres themselves are developed, and that they differ herein from the dura mater, which every one knows to be a production of the middle layer, whence the bones, and muscles, vascular and fibrous tissues are evolved. Otherwise they would have felt that they were suggesting a morphological impossibility, which would have been a pathological blunder. Or, again, would Waller have supposed that the atrophied peripheral segments of a cut nerve were regenerated by a process of outgrowth from the nerve-centres, and have left it to Lent, Schiff, and Hjelt, to show that the regeneration takes place where the first production took place, viz. *in situ*, if he had been acquainted with this fact in the history of this first production? But when we have to deal with nutritional and diathetic variations, entailing changes in the entire mass of the impartially and ubiquitously distributed animal fluids, it becomes a little

forced to refer us back to the far-off days of development for an explanation of what, as the very word "temperament" would appear to indicate, is the complex result of lengthy processes of interfusion and intermixture of factors, which, however distinct once, are most thoroughly unified now. An attempt (see Owen, on the 'Homologies of the Vertebrate Skeleton,' p. 138) to localise syphilitic or mercurial disease of bones upon the expanded neural spines of cranial vertebræ appears a little extravagant in these days, yet Dr. Laycock's claim for one or other of the layers of the germinal membrane of a predominance in settling the character of something as complex as "temperament" is conceived in much the same spirit as this curious piece of pathology.

It is of importance and interest to note that Dr. Laycock, in the now widely current doctrine of aphasia being dependent upon lesions of the posterior third of the lower frontal gyrus or convolution, and he says (vol. ii, p. 472):

"This conclusion is so contrary to the fundamental laws of bilateral symmetry with commissural activity, that on this ground alone it may be held to be erroneous, together with all the explanatory hypotheses that have been built upon it. The left side is, in fact, or much more commonly affected in other diseases of the nervous system as in aphasia, partly because of its special relations to certain visceral and functional activities, but chiefly because of certain fundamental differences in the nutrition and development of the two halves of the body, and which extend to organic life, as I showed thirty years ago ('Treatise on Nervous Diseases of Women,' 1840, chap. "On the Relations of the Lateral Halves of the Nervous System," p. 199, and 'Edin. Med. and Surg. Journal,' Oct., 1838, p. 335).

"The influence of a unilateral lesion on the faculty of speech is due to the occurrence of two distinct things, viz. the unification of the organs of speech and of their regulative ganglia and the unification of the frontal lobes in directing these ganglia."

Without pledging ourselves to the correctness or incorrectness of Dr. Laycock's views, we must say that, in a work such as the one now before us, the reader has a right to complain of not having the memoirs of the writers whom the author differs from specified to him in a bibliography, however short, as also of not having the author's own reasons expounded at somewhat greater length upon a matter of such importance.

The same remarks will apply, with more or less justice, to the following statements, which we take from different parts of Dr. Laycock's work. We give them without any other comment, leaving our readers to judge of their objective truth, and pledging ourselves merely to the inadequacy of their literary exposition. At p. 395 of vol. ii, Dr. Laycock says of Dr. Augustus

Waller's researches that they show that the morbid changes in the nerves take place in the same direction that the transient force passes. Then, upon the next page, we find him saying—

"There are some remarkable cases of *symmetrical cutaneous diseases* which may be explained according to this doctrine of the functions of the intervertebral ganglia."

The italics are ours.

Secondly, at p. 416, vol. i, we read—

"Man probably has degenerated; but man as he now exists is probably more perfect in some respects than in past time."

Much pains have been expended on Part VI, "The Principles of Mental Organology," and this portion of his work is liberally illustrated with woodcuts of the human and of some invertebrate nervous systems. And in whatever directions progress may be making, or may have been made, which will cause parts of Dr. Laycock's work—whether its illustrations, its methods, or its results—to be superseded, his statement in the following words, with which we will close this review, of the first principles of modern neuro-physiology can never become antiquated or be set aside:

"It appears that a true method of inquiry into the laws of the cerebral functions, and into the structure and mutual relations of the parts of the encephalon, can only be based upon an inquiry into the laws of life and organization, as manifested in the most general mode and in the simplest tissues, whether of plant or animal. To adopt the usual method, and begin the inquiry with the ganglia of the nervous system, is to commence with the complex; the proper commencement is with the phenomena of irritability as to adopted acts, and with the phenomena of histological change as to the processes coincident with adaptation to ends. To set animal life in opposition to plant life, or the functions of the nerves and ganglia in opposition to the functions of simple cell-tissues, is to embarrass the whole subject and render successful inquiry impossible."

VII.—Sanitary Statistics of Portugal.¹

THE present work may be regarded as an extension of the

¹ *Investigações Estatísticas sobre as Doenças e Mortalidade do Exército Português no período de seis annos e meio, decorridos do 1º de Julho de 1861 até 31 de Dezembro de 1867.* Pelo Dr. J. A. MARQUES, Cirurgião de Brigada, Chefe que Então era da 6a Repartição da 1a Direcção do Ministerio da Guerra. Lisboa, 1870.

Statistical Investigations upon the Diseases and Mortality of the Portuguese Army in the period of six years and a half, from the 1st of July, 1861, to 31st December, 1867, inclusive. By Dr. J. A. MARQUES, Surgeon of Brigade, late Head of the 6th Division of the Direction of the War Ministry. Lisbon, 1870. Pp. 125, 4to.

‘Estudios estadísticos hygienicos etc do exercito Portuguez,’ a review or summary of which appeared in the number of this Review for April, 1863, with a sketch of other writings and reports by Dr. Marques. This summary excited, as we have reason to know, a considerable amount of interest in the medical services of this and other countries, and its data have been occasionally referred to and quoted in works on hygiene. It contained the first important notice of the vesicular military ophthalmia ever published in this country, with particular warning as to the aggregation of individuals in workhouses, schools, &c., which has not fallen altogether profitless. The last decenniad of statistics closing the year 1861, forming the succeeding volume would have fallen due in proper course during the present year; but we find it maimed of its proper proportions to the extent of three years and a half; the explanation of which is as follows:—In the year 1869 the Portuguese Government was overtaken by an impulse of economy, which led immediately to the suppression of the ‘*Escholiaste Medico*,’ the chief medical periodical of Portugal, after an existence of twenty-six years, and indirectly to the withdrawal of its editor, Dr. Marques, from active service. The statistical department also, which had been conducted by him with great spirit, and we may also say disinterestedness, was subject likewise to a curtailment amounting to radical change. Of the political convulsions that followed, notoriously grave in character, we shall be silent altogether, as they concern us not. Suffice it to say that Dr. Marques, guided by the same public spirit which has animated him throughout his career, has taken on himself the publication, at his own cost, of what was originally undertaken by the Government, and the result is nothing wanting in completeness except as to length of the period embraced; nay, it is even perhaps more handsome in form and in type than would otherwise have been the case, for nothing has been spared which might serve to conciliate the reader in this thorny and not generally attractive, path of scientific investigation. We may be pained perhaps to see Portugal thus suddenly cut adrift from all confraternity in science, but must still feel indebted for the honorable position she has hitherto taken and the good part she has played in scientific medical inquiries.

The ‘Blue Books’ of Portugal are worthy, indeed, of the highest respect; among those of late years we need only refer to the remarkable report on the deleterious consequences of rice cultivation by Senh. Carvao, to the report of Senh. Gomez subsequent to the international conference on cholera held at Constantinople, as examples. The praise bestowed on Sardinia long since, that of all states it was, though in point of territory

insignificant, yet of all others the best served in diplomacy, may apply in some measure to Portugal as regards scientific investigation. No country has been more tried than Portugal by pestilence introduced from without, but she was enabled to shut out by a strict military cordon the ruthless cholera from her land while her neighbours were suffering. This shows what may be effected by a strict adherence to principles under good scientific direction, and it is more creditable since this minor state has been more than once subjected to pressure in a contrary sense by other larger communities whose views are much modified by trade. But the fact remains that during the six years and a half Portugal has escaped from pestilence while Spain has suffered most severely.

We shall now proceed to these interesting statistics, premising that from its compactness and narrow boundaries the kingdom affords an amount of area most convenient for deductions of this nature, whilst the conclusions are more perfectly verifiable than in a more scattered or broader domain. Madeira and also the Azores we shall find comprised in the returns; but not the colonies. On looking to the particular data we shall find that in chest affections some very curious deductions follow. How is it that in phthisis the troops, in town or country, are affected in such different proportions? Why in Oporto is bronchitis so prevalent and pneumonia little felt? Why in Lisbon is this last so frequent, and typhoid fever in Algarve as well as in the cities? How is it that rheumatism and bronchitis as well as typhoid fever and phthisis are so much more prevalent in the towns? Portugal has two chief cities, the capital and the port or Oporto, which exhibit these principal facts in very clear relief. These are the main subjects of interest in the reports which lie before us, and the data and figures quoted may be very justly compared with those of the preceding decenniad, and with the statistics of Barbosa, reviewed in our January number, "*Notes statistiques des grandes opérations à l'hôpital Saint Joseph. Congrès médical de Paris.*"

The army of Portugal is in strength about 21,000 men. Ten thousand, more or less, infantry; four thousand light troops, called *arcadores* or chasseurs; more than two thousand cavalry; and some fourteen hundred artillery. The *veteranos*, or serviceable invalids, recently called *reformados*, are between two or three thousand; the engineers three to four hundred. The hospital service (company of health) are more than a hundred men. These numbers vary somewhat, but not materially. The artillery are those that give the highest mortality, 14·3 deaths for 1000 effective annually; though supplying only a moderate number of patients to hospital, 66·2 to 1000 effective

The cavalry on the other hand supply 1000·4 cases per 1000 to hospital, with a mortality lower than the artillery, higher than that of infantry and chasseurs; next in order come the two last-named, either of which has a smaller number of deaths than the cavalry and artillery, but they send more cases to hospital than the artillery. The “company of health,” notwithstanding that, like the engineers, they have exceptions and indulgences more than the troops, have a high mortality and much fatal sickness, arising out of the trying nature of their service. The *veteranos* have actually one death to 5·3 entrances to hospital, as opposed to one in 100·8 of the cavalry, and 19·8 deaths in 1000 effective annually, as opposed to 10·5 of the infantry. It is an exceptional force; the engineers too are exceptional. The mortality of these latter is not high.

As to the strength of the army, the officers and sergeants amount to 2,347, the body of troops comprising corporals, *arspecados* (lance corporals) and soldiers to 18,624. Of the effective force, some 5000 are in garrison in Lisbon, 1500 or thereabout at Oporto. The movement of the sick, that is to say, entrances to hospital for each year, are 15,742, or 745·8 per 1000 of effective force; a marked improvement on the preceding decenniad. The so-called permanent number or dead weight of sick are 714·7 daily, or 34·1 in hospital for each 1000 men. The duration of treatment is estimated at 16·6 days, also an improvement. The term or duration of sickness falling to each man is estimated at 12·6 days. With respect to the garrison at Lisbon, the average of sick in each year was somewhat less than the rest of the army, 728·6 compared to 745·8, less still in Oporto, where it was only 709·2; but the mortality is higher. As to classes;—officers and sergeants gave 281·5 sick per 1000, corporals and soldiers 804·2 per 1000 annually.

The yearly mortality of the troops is 269, or 12·7 per 1000 effective, a marked improvement on the last decenniad. As regards the garrisons of Lisbon and of Oporto, in the first it is 12·5 per 1000, and in the last 17·2; in the other garrisons and rest of the army 12·2 per 1000. It will be seen then that the mortality of Oporto is the highest of all. In Lisbon the invaliding is more brisk than elsewhere, especially as regards phthysical conditions, since there is an opportunity for providing for the sick and ailing invalid in civil life. As regards the *veteranos* the mortality is really greater than that indicated in returns, since many a one goes home to die, or else ends his life in civil employ shortly after his dispensation from service, so that in fine the deaths among them cannot be estimated under 130 yearly, or 48·1 per 1000 men. In the mortality as regards classes, the officers and inferior officers show annual average of

9.3 per 1000, the corporals and line 13.1 per 1000. The proportion of deaths to sick is 1 to 58.

Intermittent fevers.—Of these there was 1 case for 9.5 of all others, or 104.1 agues for every 1000 sick, 72.8 on an average for 1000 men annually. The ancient military division, Lisbon, Leiria, Santarem, gave a third of these fevers; next to it comes the division Beja, Evora, Portoclegre, affording nearly another third, while the division comprehending the districts Braganza and Villa Real contribute approximately a fifth. The average of these agues at Lisbon was 38.3, at Oporto 38.3, in the rest of the army 90 per 1000 men annually. Here, then, we note an exemption on the side of towns. The mortality from this cause comprising pernicious and remittent fever is 3.55 per 1000 sick and 0.24 per 1000 effective annually.

Typhoid fever shows an average of 2.5 per 1000 men effective, except in the cities, Lisbon and Oporto, where it amounted to 3.2 per 1000. Similarly ague affected preferentially the military division Lisbon, Leiria, Santarem, especially Lisbon. This division gave a third of the whole, but the southern province of Algarve had the greater number of cases. The mortality from typhoid is greater in Lisbon than elsewhere: for the whole army it is 0.83 per 1000 of the force, 1 death in 2.9 cases attacked, somewhat less than in the former eight years; but as to the amount the difference is far from great, notwithstanding the precautions that have been taken against this dread plague of armies.

The cases of pulmonary tubercle diagnosed in military hospitals were 11.2 per 1000 as against other diseases. They were very numerous in Oporto and also in Lisbon. In the last-named city these cases amounted to about a third of the entire number; 365 out of 1085 in the 6½ years; giving so high a proportion annually as 11 for 1000 men effective. In Oporto it amounted to no less than a sixth of the total, in spite of the small force of the garrison, 1500 men; affording an average of 15.6 cases for 1000 men annually. It is in the highest degree worthy of note that the frequency of phthisis in the other garrisons gave an average proportion of not more than 5.2 per 1000 men annually.

These data are well worthy of remark, and the more so since, equally with typhoid fever, although much attention has been given to the subject and many suggestions adopted, nothing yet has been found to impose a limit on this mortal complaint. The observation of Lombard is still made good, that the predominance of phthisis is pretty much the same in all European cities.

The deaths from pulmonary and tubercular consumptions are

1 in 2·22 cases, or 3·2 deaths for 1000 of force. Out of each 100 cases, Lisbon claimed 36. After Lisbon the troops quartered in the province of Minho exhibited the greater number of cases, 10·8 per 100. Next those of Alemtigo 8·9 in each 100. The yearly average in the army is 72·4 deaths. The predominance of phthisis in the garrisons of Lisbon and Oporto is further explained by the following proportions:—Garrison of Lisbon 5·15 annual deaths for every 1000 of strength; of Oporto 3·04; all the garrisons besides 2·84 per 1000.

The cases of pneumonia seen in military hospitals amount to 11·8 for 1000 effective annually, with 1·23 deaths. The mortality may be accounted then at 1 in 8·91, scarce different from elsewhere. Lisbon had absolutely and relatively the largest number of deaths from pneumonia. Next after it figured the two Beiras and Traz os Montes, both of them rough and inclement and hilly, not to say mountainous districts. The province of Minho, in which Oporto is situated, has very little pneumonia; it is even rare. In Oporto the cases in hospital only amount to 5·9 per 1000 men, against 14·7 in Lisbon. In the island of Madeira there was not in the whole six years and a half a single fatal case of pneumonia; in the Azores a single case.

As regards bronchitis, the hilly districts of the Beiras and Traz os Montes show a high number, but singularly enough Oporto gives the highest proportion of cases, 78·8 for 1000 men; Lisbon 59·1; other localities 58·9: a slight but perceptible difference. The deaths from bronchitis were 1 in 86 sick—0·20 in 1000 effective. Lisbon showed the greatest proportion of deaths from this cause, as it did also from pneumonia and phthisis. Madeira and the Azores gave not one death from bronchitis. Rheumatism prevails most at Lisbon, 39·2 cases per 1000 men; next Oporto, 31·3; in the rest of the army 20·4 per 1000. The difference would seem to depend on details of service. Portugal is famed for aneurism and cardiac lesions; 301 cases were noted and the deaths were 37·5 per 100. Of hemeralopia there were 167 cases. Of granular and non-granular conjunctivitis, with purulent ophthalmia, the cases varied from 700 to 400 annually. Of purulent ophthalmia in the entire period there were 210 cases; of granular conjunctivitis 1128; of simple 2641, and no less than 316 cases of keratitis came into hospital. The invalided for conjunctivitis were 270. Army ophthalmia is not yet extinct, clinging principally to a few corps where it had spread the most (10 and 12 infantry). Variola was not remarkable. Psora was more prevalent in country districts. Of cholera we find 4 fatal cases at the frontier town of Elvas in the year 1865; in a single instance it was carried by rail to Lisbon from army quarters,

but spread no further than a family or two, with circumstances of a most singular character as regards contagion, recorded in the 'Medical Times and Gazette' of that year.

These are all the remarks we can find space for from among many interesting facts concerning the sickness and death statistics of the army of Portugal. They may serve, it is hoped, not only for present interest and utility, but perchance also for reference at some period when all that can be gathered about a foreign clime is sought for with avidity, and all seems to fall short of the end in view.

VIII.—The Physics and Physiology of Spiritualism.¹

IN our notice of this useful and instructive monograph, which has now come out at a most opportune season as a reply, by anticipation, to the forthcoming report of the committee on spiritualism, appointed by the Dialectic Society, we shall, for the most part, let the author speak for himself:

"There always have been (he observes), and probably always will be, individuals whose love for the marvellous is so great, and whose logical powers are so small, as to render them susceptible of entertaining any belief, no matter how preposterous it may be; and others, more numerous, who, staggered by facts which they cannot understand, accept any hypothesis which may be offered as explanation, rather than confess their ignorance.

"The real and fraudulent phenomena of what is called spiritualism are of such a character as to make a profound impression upon the credulous and the ignorant; and both these classes have accordingly been active in spreading the most exaggerated ideas relative to matters which are either absurdly false, or not so very astonishing when viewed by the cold light of science. Such persons have, probably, from a very early age, believed in the materiality of spirits; and, having very little knowledge of the forces inherent in their own bodies, have no difficulty in ascribing occurrences which do not accord with their experience to the agency of disembodied individuals, whom they imagine to be circulating through the world. In this respect they resemble those savages who regard the burning lens, the mirror, and other things which produce unfamiliar effects, as being animated by deities. Their minds are decidedly fetish-worshipping in character, and are scarcely, in this respect, of a more

¹ *The Physics and Physiology of Spiritualism.* By WILLIAM A. HAMMOND, M.D., Professor of Diseases of the Mind and Nervous System, and of Clinical Medicine, in the Bellevue Hospital Medical College, New York, &c. 1871. New York.

elevated type than that of the Congo negro, who endows the rocks and trees with higher mental attributes than he claims for himself" (pp. 8, 9).

After glancing at the facility with which the most careful and experienced judgment may be deceived by false personal impressions, or by non-existing images created by the mind, he adds that, as regards the latter, "the difficulty is in the brain." Alcohol, opium, belladonna, &c., will often give rise to hallucinations, and various substances act in a like manner, by their influence in deranging the cerebral circulation :

"A young lady, who had overtasked her mind at school, was thrown thereby into a semi-hysterical condition, during which she saw spectres of various kinds, which passed and repassed rapidly before her all day long. Everything at which she looked appeared to her of enormous size. A head, for instance, seemed to be several feet in diameter, and little children looked like giants. When I took out my watch, while examining her pulse, she remarked that it was as large as the wheel of a carriage. Sauvages refers to a somewhat similar case, in which a young woman, suffering from epilepsy, saw dreadful images, and to whom real objects appeared to be greatly magnified. A fly seemed as large as a chicken, and a chicken equalled an ox in size.

"Physical causes, calculated to increase the amount of blood in the brain, or to alter its quality, may give rise to hallucinations of various kinds. A gentleman, under the professional charge of the writer, can always cause the appearance of images by tying a handkerchief moderately tight around his neck; and there is one form which is always the first to come and the last to disappear. It consists of a male figure, clothed in the costume worn in England three hundred years ago, and bearing a striking resemblance to the portraits of Sir Walter Raleigh. This figure not only imposes on the sight, but also on the hearing; for questions put to it are answered promptly, and with much more intellectual force than those addressed to the so-called 'spirits.' How easy would it be for the gentleman subject to this hallucination, were he a believer in spiritualism, and less intelligent, to imagine that his visitor was a spirit, and that he held converse with the real Sir Walter Raleigh" (pp. 10, 11).

After discussing the late Baron von Reichenbach's views regarding the influence of the magnet and of the magnetism evolved by other persons on sensitive patients, and admitting, as has lately been demonstrated by Dr. Vansant, and verified by himself, "that certain very obvious symptoms are induced by the application of the magnet to the body," Dr. Hammond proceeds to show that there is no proof that magnetism, or the odic force can produce clairvoyance, the movement of tables, rappings, &c.; while, on the other hand, many of the facts of spiritualism are

clearly explained by referring them to the influence of concentrated attention upon a particular thing or part of the body, in cases when the person is of a very impressionable organisation; fancies being thus converted into realities.

“A still more important factor in the production of spiritualistic manifestations is, however, sleight-of-hand. The perfection to which this art is carried by accomplished performers is really remarkable, and is much more wonderful than would be real visitations of spirits. The knowledge of human nature and of practical science requisite for some feats of legerdemain is necessarily very great, and the manipulations often require a degree of dexterity which cannot fail to excite astonishment. It is a well-known fact that the art in question can be satisfactorily made to explain many operations apparently supernatural, and that ‘mediums’ do not hesitate to avail themselves of its resources. They are most of them, however, sorry performers when compared with East Indian jugglers, and with some who make no secret of the fact that their performances are deceptions.

“A short time since I invited several medical and other friends to witness, in my library, some surprising spiritualistic exhibitions by a first-class ‘medium.’ The operator went through all the performances of the Davenport Brothers, to the entire satisfaction of the audience. He was securely tied by a gentleman who had been an officer in the naval service, and who exhausted his strength and ingenuity in devising bands and knots. A screen was then placed in front of the ‘medium,’ and in an instant an accordion was played, a bell rung, and a tambourine struck. The performer then requested that the screen might be removed, and, on this being done, he was found to be tied in precisely the same manner as at first. The gentleman who had bound him declared that not a cord or knot had been interfered with. In a second attempt the ‘medium,’ tied with additional care, rang a bell, and was discovered intact in a second afterwards.

“The ‘rapping’ of this gentleman was perfect, and he read communications from the dead, made on folded slips of paper, with a skill equal to that of the most orthodox and highly-gifted medium.

“The astonishment of the audience was great when he informed them that all his performances were deceptions, which he then proceeded to explain in the most satisfactory manner” (pp. 20, 21).

We should be glad if this gentleman, who rejoices in the remarkable name of Dr. von Vleck, would pay us a visit to England.

In his fourth chapter Dr. Hammond gives us a most instructive sketch of somnambulism, both natural and artificial, including Mr. Braid’s ‘hypnotism.’ The persons who are the most readily hypnotised are of the same nervous class as those who yielded to the influence of Von Reichenbach’s odic force, and they are now found to form the best mediums; and a person

in the hypnotic state who "should be disposed to deceive, or should be under the control of designing persons, could not fail, says the writer, to be received by many as a medium of the first order."

From the careful consideration of a very remarkable case of combined hypnotism and pure somnambulism occurring in a young lady whom he carefully watched for a long period, and of others similar to it, that have fallen under his observation, Dr. Hammond comes to the conclusion that in hypnotism there is a combination of hysteria, catalepsy, and ecstasy, and to each of these diseases, in so far as they illustrate the subject of spiritualism, he devotes a separate chapter.

In the first of these chapters numerous instances are collected, in which hysterical women have deliberately entered upon a course of deception, often involving personal discomfort, amounting, in some cases, to what must be agonising torture, if the sensibility were intact, solely for the purpose of attracting attention and obtaining notoriety. He has witnessed, in his own practice, cases rivalling those of the Jansenist Convulsionnaires, and the possessed nuns of Lowdon; and he quotes several remarkable phenomena, recorded in a recent book, which is regarded as a good spiritualistic authority (Mrs. Hardinge's 'Modern American Spiritualism,' second edition, 1870), and amongst them the following :

"Four silly, badly-educated girls, of ages ranging from fifteen to twenty, having gathered together at a friend's house to 'have a time with the spirits,' or, in other words, to trifle with spiritual manifestations, seated themselves around the table, and, after asking all manner of foolish questions, requested the spirits to take hold of them.

"The spirits at once complied, seized them, treated them in the roughest manner, and, shaking them, caused them to use the most violent actions and outrageous language, &c. In this strait one of the dignitaries of the mother Church was sent for in haste to 'expel the obsessing demons.' After the priest had arrived at the scene of disorder, he put on his robes, got ready the holy water, and approached the possessed girls in the due formulæ proper to such occasions. After many sallies with the holy fluid, and a vast number of incantations, none of which produced the slightest effect, the mediums at length charged upon him with such irresistible power, and such capacity of finger-nails, that the worthy *padre* fled precipitately, leaving the field in possession of the 'demons' and the spectators who had gathered together to witness the 'exorcism.' The girls still continued to be used roughly by the discordant spirits they had invoked, until the arrival of some of their spiritualistic friends, by whose judicious passes and gentle remonstrances with the spirits they were instantly relieved."

"That these 'silly, badly-educated girls' were simply hysterical, no one with even a superficial acquaintance with the normal condition of the nervous system, and the aberrations to which it may be subjected, can entertain the slightest doubt. It is from just such persons as these that the best mediums are obtained. That such phenomena as they, and the girl whose case was previously quoted,¹ exhibited are regarded as spiritualistic, is sufficient of itself to throw discredit on all the other alleged manifestations of the spirits. *Falsum in uno, falsum in omnibus.*

"At most of the spiritualistic meetings which the writer has attended there have been hysterical phenomena manifested by some of the men and women participating in the exercises. At a recent public exhibition of the kind he predicted, from their personal appearance, with perfect accuracy, who of those assisting would be thus affected. The symptoms of disordered nervous action, which the audience was invited to consider proofs of spiritual agency, consisted of incoherent utterances, and convulsive movements of the head, arms, and legs. In one case these symptoms became permanent for several months; a well-developed case of chorea or St. Vitus's dance was thus established. The patient finally came under the writer's care, and was only cured by the persistent administration of iron and strychnine, medicines which, with good food and fresh air, appear to possess more exorcising power than the formulæ of the good priest mentioned by Mrs. Hardinge" (pp. 49, 50).

The records of spiritualism abound with instances of spirits being seen by the faithful. Dr. Hammond proceeds to show from cases occurring in his own practice that hysteria, independently altogether of spiritualism, affords similar examples.

"A young lady gave very decided evidence of suffering from mental aberration. She had imbibed the delusion that she had a 'double,' whom she saw almost constantly, and with whom she conversed whenever she pleased. At first she had been very much frightened, but gradually had become accustomed to her imaginary companion, and was lonesome and uncomfortable without her. There was no other well-marked delusion, though some of her absurd fancies partook more or less of that character. Her menstrual function was deranged, and her whole aspect was that of a person whose physical powers were below par. Strychnia, iron, and whiskey, and a full nutritious diet, soon dispelled her delusional visitor, and otherwise established her health.

"A married lady consulted the writer for advice regarding hallucinations of sight and hearing, with which she had suffered for several months. It was only necessary for her to think of some particular person, living or dead, when she immediately saw the image of the person thought of, who spoke to her, laughed, wept, walked about the room, or did whatever other thing she imagined.

¹ Reference is here made to a girl described in an earlier part of Mrs. Hardinge's book, who was by turns under the influence of a good and a bad spirit.

In fact, to such an extent had her proclivity reached, that it was often impossible for her to avoid thinking of persons, and immediately having their figures brought to her perception.

"At first she religiously believed in the reality of her visions, and that she really saw the spirits of the various individuals of whom she happened to think. But, as the hallucinations became more common she lost her faith, and ascribed them to their true cause, disease" (pp. 51, 52).

Dr. Hammond goes on to state that this lady was pre-eminently of an hysterical type; and that under suitable treatment she entirely recovered.

Passing, for want of space, over the chapters on Catalepsy and Exstasy, and on the power which certain persons have of voluntarily producing hallucinations of various kinds, until at length they cannot get rid of their false perceptions, we come to the consideration of the important question of *Spiritualism* versus *Gravitation*. The faculty of "levitation," or rising in the air against the force of gravity, has been claimed not only for tables and chairs, but for the human body. Long before the spiritualistic era saints of the Roman Catholic Church were in the occasional habit of being lifted by unseen angels from the earth. Dr. Hammond gives a list of those highly favoured individuals, all of whom were "new saints," in pp. 68—74; and he subsequently notices cases in which similar results have been produced by demons or witches.

"In all supposed instances of levitation such as have been cited, the true explanation may be found by referring them to one or other of the following causes:

"1. *An hallucination on the part of the subject, or of those asserting themselves to have been witnesses.*—As De Boismont remarks: 'The sensation of flying is rather common. Frequently, in dreams, we feel ourselves borne along with the rapidity of an arrow; we accomplish great distances, just lightly touching the ground.' We have noticed this fact in a literary man of our acquaintance, whom we have several times found with fixed eyes, and who said to us, 'I am flying; do not stop me.' On returning to himself he described his sensations, and it seemed to him that he really had flown" (p. 75).

"Numerous other instances of similar cases are recorded in works, on psychological medicine, and several have come under my own observation.

"In one of these, a lady of strongly marked hysterical temperament, and of almost fanatical religious tendencies, imagined that she was frequently raised from the ground while in the act of saying her prayers. She usually spent several hours each day in these exercises, and during the whole time was in a state of fervid exaltation, which rendered her insensible to all that was passing around her. While in these conditions she would exclaim, 'I rise! I rise! I see angels!'

and with her hands raised on high, her head elevated, her face turned upward, and her countenance illuminated with ecstatic radiance, she really did seem, to some sympathetic and superficial observers, to be lifted up. Among others, her maid was strongly convinced that the elevation was actual; but stronger minded members of her family could see nothing of the kind, and eventually the lady herself became convinced that she was the victim of self-deception. A young married lady, now under my professional care, is very confident that, during the cataleptic seizures to which she is subjected, she is raised from her bed, and she appeals with confidence to those surrounding her to confirm her statements. It almost always happens that some one present expresses the opinion that she really was lifted up several inches.

"The majority of the cases met with in the lives of the saints belong to this category. Nearly all the subjects were the victims of some severe disorder of the nervous system, by which they were rendered peculiarly susceptible to hallucinations; and their more ardent followers were either similarly affected, or were so impressed by the power of suggestion, already considered in this essay, as to be fit recipients of erroneous mental or sensorial impressions.

"The appearance as if about to fly is very common in cases of ecstasy, and is due to the raising of the arms, the upward look, and the elevation of the body on the extreme points of the toes. This position is sometimes kept for hours, and may readily—as the stature is increased in height—lead to the opinion that the body is off the ground, especially in the cases of women, whose feet cannot readily be seen, owing to the drapery of their dress" (pp. 76, 77).

"2. *Unintentional exaggeration, misinterpretation, and inaccuracy of statement*" (p. 77).

"3. *Insufficient evidence*.—Most of the instances of levitation which are recorded rest on insufficient evidence, such as would be inadequate to establish the fact in a court of law. This is the case, for example, with several of the alleged instances occurring in the persons of saints, monks, and nuns. It does not appear that Calmet ever saw the human body lifted up without material agency, although he refers to several cases of which he had heard. Hearsay testimony is of so equivocal a character as to be disregarded in all matters of importance, and yet we are expected to rely on it as sufficient to establish the fact of miraculous events, which, of all others, should require the most unerring and irrefragable demonstration" (pp. 78, 79).

"4. *Intentional misstatement*.—Some of the instances are probably due to misrepresentation, with the view of enhancing the reputation for sanctity of the subjects, or simply from that love for telling marvellous stories which is so inherent in mankind; such, in all likelihood, are the earlier examples—as that of St. Dunstan.

"The cases, too, recorded in connection with withcraft are many of them clearly fraudulent in character, fabricated for the purpose of injuring some obnoxious person by the imputation of being a witch" (p. 80).

"5. *Legerdemain*.—The Braminical exploits of rising in the air to the height of two cubits, and walking then without earthly support, made before Apollonius Tyaneus,¹ may be thus explained.

"The modern instances, attributed directly to the influence of the spiritualists, are scarcely deserving of mention. None of them are well authenticated, and all are more reasonably explained by ascribing them to one or several of the causes specified. Performed in the dark, they afford abundant opportunity for deceit on the one part, and hallucination or illusion on the other. They do not even claim to be as powerful manifestations as those specially referred to in this essay, for the latter were asserted to be done in broad daylight, and the subjects could be touched by those present; while those of our day avoid inquiry, and are performed under such circumstances as to defy thorough examination" (p. 81).

We conclude our notice of this most interesting little book with what are virtually the last words of the author.

"To describe in detail all the vagaries of spiritualism would be a fruitless undertaking. He has witnessed many spiritualistic performances, and has never seen a single one which could not be accounted for by the operation of some one or more of the causes specified. No medium has ever yet been lifted into the air by spirits, no one has ever read unknown writing through a closed envelope, no one has ever yet lifted tables or chairs but by material agencies, no one has ever been tied or untied by spirits, no one has ever heard the knock of a spirit, and no one has ever spoken through the power of a spirit other than his own.

"Even if bodies had been raised in the air by agencies unexplainable, even if some one had read writing through several thicknesses of paper, even if others had been bound and unbound in a way unknown to us, even if knocks had been heard whose sources could not be ascertained, even if the causes of all the phenomena of spiritualism were entirely beyond our present knowledge, there would be no proof that spirits had anything to do with them. On the contrary, the hypothesis of spirits is altogether the least plausible which could be suggested. The phenomena and the explanation have nothing in common.

"Spiritualism is a religion. As such, it is held tenaciously and honestly by many well-meaning people. To reason with these would be a waste of words, just as much as would be the attempt to persuade a madman out of his delusion. Emotion, or interest, or action might change them, but facts never. But there are some who halt between belief and unbelief, for the reason mainly that they have no clear conception of what knowledge is, and of how things are to be proved" (pp. 84, 85).

Although spiritualism is, as yet, not nearly so rampant in our own country as in America, the believers in it and its delusions

¹ Philostrates, 'Vita Apollonii Tyanei,' lib. iii, ch. xx.

are even here daily increasing. It is hard to understand how it can enthral the intellects of such men as Wallace, of Darwinian fame, and Varley, the eminent physicist; but such is the lamentable fact. We are not aware that any members of our own profession have as yet been deluded; and it is not so much on their account as that of their patients that we have given this prominence to this sensible and convincing little book. To any weak-minded person who may show signs of nascent contagion we would say, Buy and study 'Dr. Hammond's Essay on Spiritualism.'

IX.—Indian Medical Reports.¹

INDIA presents a magnificent field for the investigation of many epidemic diseases. Its vast extent of territory, of more than thirty degrees of latitude, from near the equator to the regions of eternal snow, and of nearly as many degrees of longitude within the tropics, having the most diversified characters of geography and climate, and its enormous population of different races, under the most varied circumstances of life, afford admirable opportunities for the study of the natural phenomena and of the social conditions which accompany their development and diffusion. Much, happily, is now being done to turn to useful account this mighty field of research. An accurate mortuary registration has been established throughout the branches of the public service, and the system is gradually being extended to the civil population, notwithstanding the great difficulties of the undertaking. The medical and hygienic supervision of different regions is becoming more complete through the machinery of provincial sanitary commissioners and of local inspectors. In the annual reports of the former much most valuable information is gathered together, and these reports are afterwards examined and summarised by the government of each presidency, from one year to another. Finally,

¹ 1. *Sixth Annual Report of the Sanitary Commissioner with the Government of India*, 1869. Folio, pp. 263. With Statistical Returns. Calcutta, 1870.

2. *Report on the General Aspects of Epidemic Cholera in 1869*. By JAMES L. BRYDEN, M.D. Folio, pp. 66. Calcutta, 1870.

3. *Report on the Sanitary Administration of the Punjab*, 1869. Folio, pp. 255. With Death Returns. Lahore, 1870.

4. *Administration Report on the Jails of the Lower Provinces for 1869*. By F. J. MOUTAT, M.D., &c. 4to, pp. 266. Calcutta, 1870.

5. *Report on Measures adopted for Sanitary Improvements in India, from June, 1869, to June, 1870*. Printed by Order of the Secretary of State for India. Folio, pp. 292. London, 1870.

the general superintendence of the entire work by the India Board and the War Office at home serves to connect the presidential digests into one great whole, whereby uniformity of procedure is maintained, and the generalisation of results over the entire peninsula most satisfactorily secured. 1869 was emphatically an epidemic year, and in this respect contrasted strongly with the previous one, which is stated to have had "a peculiarly non-epidemic character." It was, consequently, a very sickly and fatal year among both Europeans and natives. Cholera was widely spread over almost the entire presidency, and caused immense losses of life in many localities. From Assam to the line of the Indus and beyond the frontiers of Cabul in a north-west direction, and right across the peninsula to Bombay in a south-west direction, few districts of any great extent escaped the breath of the pestilence. Nor was it cholera alone which abounded during the twelve months; there was also an enormous amount of epidemic fevers throughout the land, and the prevalence of dysentery and diarrhœa, of hepatitis, of smallpox, and of apoplexy, was likewise unusually great. There had not been such a sickly year among the troops since 1863, and the death-rate, too, was higher than it had been for many years. We still remain profoundly ignorant of the secondary or physical causes of these irregularly returning cycles of great and wide-spread sickness and mortality. Their connection with the meteorology of the year cannot yet be at all satisfactorily made out, although there is much reason to believe that when our knowledge of this difficult department of science becomes more accurate and enlarged various inter-relations between different branches of physical research will be discovered to reward the labours of patient inquiry. It is only by the continuous and systematic registrations of exact observations over various and sufficiently ample fields, in the first instance, and then by the rigorous scrutiny of these observations, and the unhasty eduction of the general truths to which the data thus obtained clearly lead, that such discovery can be hoped for.

In our number for July last we gave a brief sketch of the general course of epidemic cholera over the Bengal presidency during the successive years from 1854 to the beginning of 1869. At present we propose to continue this consecutive narrative, and follow the career of the pestilence during the latter year, when, as we have already said, it acquired immense diffusive energy, and extended beyond the confines of India to Persia, East Africa, and the South of Russia in Europe. In 1868 the seat of occupation in Bengal is represented on the map by a broad belt of country, stretching from the north of the province of Oudh across the whole breadth of the peninsula to Malwah

and the coast of Bombay. The whole of the northern and central provinces of the presidency were then unscathed. In the spring of 1869 the disease not only reappeared over the area which had been occupied in the previous year, but it soon began to extend beyond its northern limits, manifesting its presence in various places in Rajpootana and the central provinces, which before long were almost universally involved, so to speak, in the pestilential cloud. Subsequently it became developed more and more extensively in a northerly and westerly direction, until, towards the end of the year, it had invaded by far the greater part, with some remarkable exemptions, of Northern and Central India. As in all former epidemics, the extent and severity of the visitation differed much in different districts and provinces, from causes which mostly elude our ken.

It will be useful rapidly to survey the behaviour of the disease in different regions of the presidency, beginning with lower Bengal. This region, embracing, as it does, the deltas of the Ganges and of the Brahmapootra, has, it will be remembered, peculiar topographical characteristics, which give it a climate of its own. It mainly constitutes what Bryden and some other Indian authorities regard as the endemic area of cholera, where the morbidic poison originates, and whence it is disseminated to the surrounding regions. The disease was far from being so general or so destructive there in 1869 as in many former years. The European troops suffered but little at any of their stations, and the European garrison of Fort William, numbering about 1100 souls, near Calcutta, escaped altogether, this exemption being ascribed to recent sanitary improvements, of which a good water supply is one. The native portion of the garrison, 600 strong, lost three men. At several of the stations occupied by native troops alone, as at Dacca in the delta, and at Newgong and Debroogur, in Assam, there were numerous attacks, and a good many deaths.¹ In Calcutta the proportional death-rate from cholera was about twice as great among the Hindoo and Mahommedan population as it was among the Christian inhabitants. The mortuary registration is not yet sufficiently advanced in other parts of the lower province to enable the mortality to be ascertained among its general population.² In the numerous prisons the disease was certainly less fatal than usual in them, with the exception of two or three only.

¹ At the mountain station of Shillong, in Assam, 5000 feet above sea level, there occurred ten cases—three fatal—among the native troops, and no fewer than eleven deaths among a body of prisoners only fifty-eight in number.

² Dr. Mouat says, that "the year 1869 was a time of exceptional severity in regard to the incidence of epidemic cholera in Lower Bengal. In some districts it swept away a large proportion of the population."

In Assam and the other districts lying eastward of Bengal proper, cholera was certainly very prevalent in 1869. Beyond the general fact little more can be said on this head. We shall, therefore, proceed northward and westward from the lower region along the valley of the Ganges to the provinces of Bahar, Oudh, and the lower part of the Jumna-Gangetic Doab, embracing an immense area of great fertility and wealth, and in which the populous cities and important military stations of Patna, Dinapore, Benares, Mirzapore, Allahabad, Banda, Cawnpore, Lucknow, and Fyzabad are situated. Throughout nearly the whole of this vast tract the pestilence, which had occupied a large portion of it nearest to Bengal in the previous year, prevailed in force in the spring and summer of 1869. The mortality among the civil population, from cholera alone, was estimated at between 60,000 and 70,000 deaths at least. The European force, about 11,000 strong, lost 250 of its number, while among the native troops, over 7000, the deaths were only 19 in all. At the great military and civil station of Allahabad, at the junction of the Ganges and Jumna, the European garrison, and especially one regiment, the 58th, suffered very severely. From the beginning of the year the city, which is close to the cantonments, had been infected. The earliest cases among the troops occurred about the end of March, and from that time to the second week of August the disease was never, with the exception of one interval of exemption between June 11th and July 22nd, the period which marks, according to Dr. Bryden, the separation between the spring and monsoon cholera in Bengal, absent from the regiment. During this long suffering the troops were repeatedly moved from one site to another, but without, it would seem, any decided benefit. The regiment (which was nearly decimated by the scourge, while the native force in the station lost scarcely 1 per cent of its number) had arrived at Allahabad only at the beginning of the year, one wing from Benares, in the Gangetic Valley, the other wing from the mountain station of Darjeeling, where it had enjoyed excellent health. The latter wing suffered much more severely than the former. Moreover, a draft, recently arrived from England, experienced very heavy losses. At Lucknow, also, as at Allahabad, the comparative exemption of the native part of the garrison, while the European part suffered greatly, was very marked. The population of the city, too, which is quite near to the quarters of the military, were but little affected. Among the large body of prisoners in the numerous jails scattered over the region we have been now considering, the rate of mortality from cholera during the year, although it considerably exceeded that among the native troops, was immensely below that among the

European soldiers. The total deaths amounted to 129 out of a force numbering over 20,000 prisoners. In the great majority of instances the earliest attacks in the jails could not be traced to any communication with infected persons or localities; they seemed to occur quite independently of any such intercourse.

Before we advance further northward towards the upper provinces, let us look, on the map, at the vast region that lies between the line of the Jumna on the east, and that of the Indus and the territory of Scinde on the west, and which comprises Rajpootana, Malwa, and the central provinces, between the 20th and 27th parallels of latitude. Throughout most of this immense tract cholera, which in the previous year had been limited to the southern part only of the region, was extremely prevalent in 1869, and caused very great mortality. Among the general population the deaths were estimated at between fifty and sixty thousand at the very least; in 1868, the loss of life from the same cause was considerably under ten thousand. The European troops suffered most severely at Gwalior and its neighbourhood; also at Saugar in Central India. It was in the Gwalior district, too, that the largest number of deaths occurred in the native force. The European garrison in Agra (and which is about fifty miles north of Gwalior, and between which there is frequent intercommunication) upwards of a thousand strong, lost only one man; the native garrison, less numerous, had however seven deaths. Among the prisoners in the numerous jails over the extensive region under notice was considerable; chiefly in the jails in the southern districts, as at Jubbulpore, Nagpore, and Chanda in the central provinces. The Sanitary Commissioner of these provinces was of opinion that prevalence of the epidemic in any locality was mainly dependent on the insufficiency and impurity of its water supply, and also that its dissemination was largely due to the extent of the traffic and intercourse between infected and uninfected places. On the latter point, the resident civil medical men were at issue with the Commissioner; nor were they at all agreed respecting the alleged influence of the former agent. The severity of the visitation in and around Gwalior seems to have been greatly aggravated by the general sickness of the season, and the effects of the famine consequent on the extraordinary summer heats. Between June and October, nearly 40,000 persons died of starvation; and the deaths from disease, mostly cholera and heat apoplexy, were estimated at 90,000. The cattle also died in immense numbers. A similar state of things existed throughout a large part of Rajpootana; thousands died of starvation, and multitudes who escaped this death fell victims to the wide-spread pestilence. The

disease appeared almost simultaneously in many distant points over the extensive infected area. Dr. Moore, the sanitary officer of the region, came to the conclusion that human intercourse could not explain the irregular outbreaks of the epidemic, although it seemed on several occasions to contribute to its partial and local dissemination.

In marked contrast to the two last-mentioned regions of the Bengal presidency, in respect of the general diffusion of cholera, was the province of Rohilcund, together with the district around Delhi on the west, and the hill states to the north, all of which region remained comparatively unscathed by the epidemic. This region contains many important military stations, as those of Meerut, Bureilly, Delhi, Moradabad, Seharampore, and Alligur. Three fourths and more of the fatal attacks among the European force throughout the entire area occurred at one station, that of Meerut. The native troops in that cantonment escaped altogether; and so did the prisoners in Meerut jail, although the population of the town suffered pretty severely.

At the great city of Delhi, neither the troops nor the prisoners were at all affected; and the town itself was very slightly touched. Of the eighteen hill stations, or sanatoria for European troops, one only was attacked, viz. Subatoo (about twenty miles from Simla, and at a considerably lower elevation), where the 41st European regiment lost, in the latter half of August and first week in September, 19 out of 28 cases. One fatal case in a man who seems to have recently come up from the plains seems to have occurred at Simla prior to the outbreak at Subatoo. Whether there was any connection between the two events was not determined, or it must have been from want of due scrutiny of all the circumstances at the time of their occurrence. The adjacent stations of Dagshai and Kusserobe remained intact.

There now remains but one other region to be noticed in the history of the epidemic of 1869 in the Presidency, that of the Punjab, and the Trans-Indus districts of Peshawur, Kohat and Bunnoo, which lead on to Affghanistan. It was on these districts that the stress of the visitation in Northern India fell; for, with the exception of Umritsur and the adjacent tract of country towards the city of Lahore (about twenty miles distant) in one direction, and towards the Himalayas in another, the whole of the Punjab was comparatively unaffected. The troops, European as well as native, nearly escaped scot free; one or two cases of deaths only occurring in the cantonments of Umballa, Jullundur, Sealkote, Ferozepore, and Mooltan. At Umritsur, the small European garrison lost seven men out of nine who were attacked: the native force there escaped; but the prisoners suffered considerably, and the town itself was severely scourged,

the deaths (which occurred mostly in August) amounting to three thousand at least, out of a population of 134,000. The history of the epidemic in the city and neighbourhood of Lahore, is of especial interest. The district generally was lightly touched; the city and civil station suffered more than other parts. There were 184 deaths in the former, and 71 in the latter. The greatest mortality occurred in the lunatic asylum, which lost 33 out of 200 inmates during September. On this visitation the great military station of Mean Meer, with its garrison of between two and three thousand troops, European and native, only four miles distant from the city of Lahore, and which suffered so disastrously in 1861 and in 1856, remained exempt.

“From its position, it was certainly in the vicinity of great danger, but not a single case occurred among its garrison or among the people of its bazaars. Latterly, canal water which had been introduced into the station was used for drinking; but (says Dr. Cunningham, the Sanitary Commissioner with the Bengal Government) I am not inclined to attribute much of the result to this change. The water runs in an open cutting, and is thus very liable to pollution. Between the 16th and 19th June, and again from August 21 to September 21, a cordon was drawn round the station, and no ingress or egress was allowed without leave. But the number of passes required for grass cutters and other camp followers was so large that, beyond inspiring confidence, little good could have resulted from this attempt at isolation of the cantonment.”

It is when we cross the Indus that the signs of the epidemic in deadly force becomes apparent. The earliest cases in the Peshawur valley occurred in villages close to the cantonments, in the latter half of August. On September 1st the disease broke out in the town of Peshawur, and rapidly spread, not only among its inhabitants, but to other parts of the district. During the month its ravages were very great, especially in the town. It then declined as rapidly as it had increased. The neighbouring villages also suffered very severely. Of 31 attacks in the jail at Peshawur, 13 were fatal. The native population, also, of the bazaars belonging to the cantonments, suffered much. The first case among the latter occurred on the 4th September, and it was during the remainder of this month that the main violence of the epidemic was experienced. When cholera appeared at Peshawur, the European garrison (including women and children) numbered 2050 souls, and the natives 3380. Of the former, 241, and of the latter, 111 perished—an enormous mortality. The disease was of the most virulent type, and the death rate to the number of cases was extremely high. It was in the first and second weeks of September that the attacks commenced in most of the corps, and the greatest

ravages occurred during the rest of the month. The garrison of 32 European and 75 native soldiers that occupied the small fort close to the city almost entirely escaped. "Although very careful inquiries were made at the time, all attempts to trace the disease in the Peshawur valley to importation failed." A few cases occurred in the latter half of September at the station of Nowshera, situated close to the high road leading to Peshawur, between that town and the Indus. No communication between the two places could be traced to account for the first case in the former, and which occurred on the 16th. To the south of Peshawur lies the valley and town of Kohat, and to the south of that is the station of Bunnoo. The earliest cases of cholera in the former place occurred at the end of September;¹ the ravages were very great among both the troops (native) and the general population, during the following month. To prevent the disease from reaching Bunnoo, a very strict quarantine had to be kept up; but without avail, for the disease appeared in the town and in the cantonment there in the first week of November, and continued for two or three weeks, but much less fatally than at Kohat.

Our knowledge of the history of the epidemic beyond Peshawur towards Caubul is extremely imperfect. It seems to have appeared at Tellalabad, in the Khyber pass, in September or October, and at Caubul about nearly the same time, or very soon afterwards. But far west of this region the morbid influence had been felt simultaneously, if not at an earlier date. Cases of cholera occurred at Meshed, and other places in the west of Persia, and also at Astrabad, on the Caspian, in September. During the same month the south as well as the east of Persia was the seat of the disease. The towns of Shiraz, Ispahan, Bagdad, and Bushire, among other places, suffered more or less severely. Synchronously with these events on the Asiatic continent, it appears that in Kiev, and other southerly provinces of Russia, diarrhœal and choleraic affections, which had been more than usually prevalent during the summer, became epidemic, and assumed the features of genuine cholera about the middle of September.

"At first it was supposed," says Dr. Bryden, "that the cholera had reached Kiev from Persia; but it appears that this year the

¹ Dr. Kelly, who was in medical charge of the 1st Punjab Infantry, at Kohat, says that ague, accompanied in some cases with atonic flux, was very prevalent there during the whole of September. Both in that and in October "several cases of a peculiar form of disease occurred, the chief symptoms of which were thin watery stools, vomiting of a clear fluid, and some degree of collapse. These symptoms mostly occurred in the subjects of the fever, constituting the 'pernicious fever' of some writers. The symptoms closely resembled cholera, but they are distinct and different diseases."

great town of Tiflis, on the high road between the two countries, has been unusually healthy, and that there is direct communication between Kiev and the Persian districts where cholera prevailed."

There is nothing in the history of epidemics that deserves more sedulous exploration than that of the apparent *synchronism* of their manifestations in different districts of the same country, or in different, and it may be distant, countries. It is a point that should command the earnest attention of all who are interested in tracing the career of these migratory diseases. It was towards the end of October of this year that the east coast of Africa again became the scene of cholera invasion. The destructive outbreak at Zanzibar occurred in the following month. For particulars respecting the history of this and of former visitations in that part of the African continent, the reader must consult the highly interesting paper by Dr. Christie which was read before the Epidemiological Society, and is published in the *Lancet* for January 28th and February 11th of the present year.

And now what lessons do we learn from the history of the cholera epidemic of 1869 in Bengal, in respect of the influences or agencies which seemed mainly to affect its development and progression? Dr. Cunningham wisely abstains from committing himself to any particular etiological doctrine or doctrines. His duty clearly, in the position as sanitary adviser of the government, he holds is to collect and record the most authentic intelligence he can obtain from all sources, carefully to weigh and compare the evidence of other observers, and also the opinions which may accompany this evidence, apart from every bias, or any regard for names or authorities, and then to discriminate to the best of his power all real or actual facts, and all ascertained truths from mere probabilities, however plausible or ingenious. His part is obviously to act as a judge, not as a partisan or advocate, in the matter.

Dr. Bryden's views, as explained in our article of last year, are necessarily still largely speculative; but they well deserve more than ordinary attention from the immense field of statistical observation continually under his review, and the untiring industry and great ability with which from year to year he explores it. He points out how the epidemic of the previous year, 1868—which was comparatively very limited in its area of diffusion, occupying exclusively the southern portion of the presidency, and leaving all but intact the whole of the vast extent of Central and Upper India—became resuscitated, so to speak, over its former area in May of 1869, and soon afterwards began to manifest its movements in a northerly and a north-easterly direction. About the middle of June, Dr. Cunningham,

at the suggestion of Dr. Bryden, addressed a letter to the Government in India, expressing his apprehensions, in consequence of the recent appearance of cholera among the troops at numerous stations from over an immense area, from the southern provinces on to the confines of the Punjab, that it might prevail in force later in the season over all Northern India, as it had done, under apparently similar circumstances, in 1856, and again in 1861.

"There is great reason," he remarked, "to fear that the troops in the following stations will be invaded, and that in those where the disease has already appeared it will become more purulent in the coming months of July, August, and September:—Allahabad, Cawnpore, Lucknow, Meerut, Delhi, Muttra, Agra, Morar, Ihansi, Sungor, Jubbulpore, Umballa, Ferozepore, and Mean Meer."

In his letter he prudently admits that, with our yet very imperfect knowledge, it is extremely difficult, if indeed possible, to forecaste the career of epidemic cholera in any year; but he felt it, he said, his duty, judging from the antecedent history of the disease in recent years, to warn the Government of what seemed to him to be an impending danger, in the hope that the timely adoption of precautionary measures might diminish, if not avert, the recurrence of the disastrous results on some former occasions. Happily, the forecaste was not verified in respect not only of the three last-named places, but also of Delhi and Agra. The escape of Mean Meer, within four miles of Lahore, where the disease existed for some time, although not very severely, was especially notable, as "this was the first occasion," according to Dr. de Renzy, the Sanitary Commissioner of the Punjab, "since it was occupied, that the cantonment escaped cholera when the disease was epidemic in the neighbourhood." We have failed to make out distinctly Dr. Bryden's explanation of the fortunate event, or to discover whether the exemption of the locality accorded with his theory of the development and course of the epidemic. Dr. Bryden is at times anything but very lucid in his language. Dr. de Renzy suggests its probable cause by remarking that "this was the first occasion also that Mean Meer had a tolerably safe water supply." This gentleman is so ardent a disciple of the water-propagation doctrine in respect of cholera, that his report throughout shows that he is continually striving to discover proofs or probabilities in favour of his favourite hypothesis. Candidly acknowledging that his "facts are no doubt meagre, owing to the difficulties incidental to such inquiries in India," and also that the origin of the outbreaks in various places in his province attacked in 1869, as at Peshawur and Umritsur, could not be traced to the importation of cases from infected localities, he seems to rest his belief mainly

on the circumstance that the doctrine in question has been countenanced by some eminent sanitary authorities in this country,¹ and is "almost universally accepted by medical men and the English public generally." As to the latter statement, he is, we need scarcely say, quite mistaken; and even if it were not, the mere general acceptance of an improved hypothesis can never be taken as a safe basis in scientific research. When we are told that "through the bath, on butter, on uncooked vegetables and fruit, and, finally, in drinking water, it (the choleric poison or 'cholera stuff') has innumerable facilities for reaching its breeding-ground in the stomach and bowels," and "that cholera in an epidemic form is so easily preventible that its presence will soon come to be regarded as a disgrace to a civilised Government"—the preventive being a pure water supply—we might surely expect some appeal to be made to published results of experience and observation in India, and elsewhere, as a reasonable ground for such conjectures. Dr. Cunningham, from an examination of all the evidence that has come before him respecting the epidemic of 1869, concludes that, while in a few instances there appeared to be some connection between the severity of an outbreak and the impurity (possibly from excretal pollution) of the water supply, the indications of such a connection were far from being generally present. The experience of the year was also opposed to the belief that human intercourse was a principal cause in promoting the dissemination of the disease. "From nearly every cantonment, as well as from nearly every jail, the statement is made with a sameness which is almost monotonous, that no communication, either direct or indirect, could be traced between the person first attacked and any previous case." Very generally, too, the attendants on the sick escaped, a point which is repeatedly dwelt upon by Dr. Monat also. Dr. de Renzy, notwithstanding his acceptance of the conclusion of the Constantinople conference, that human intercourse is indispensable to the transference of cholera from one place to another, admits its signal failure, in the Punjab, of quarantine measures to arrest the pestilence, and that they have often occasioned much unnecessary annoyance and suffering to the population of the surrounding districts. Dr. Cunningham confirms this statement in respect of other provinces of the presidency, and Dr. Monat tells us that "quarantine failed utterly in the only place in which it was tried, with the care and precaution necessary to test its value." Pettenkofer's views about the influence of "ground water" on the diffusion of dis-

¹ (1) Dr. Farr, 'Report on the Cholera Epidemic of 1866 in England.' (2) In the Report of the Medical Officer of the Privy Council. (3) Address of Sir W. Jenner at the Meeting of the British Medical Association at Leeds in 1869.

ease seem to be all but universally repudiated in India. We shall not at present notice the elaborate report by Dr. Lewis, 'On the Microscopic Objects found in Cholera Evacuations, &c.,' but reserve the consideration of this subject for another occasion, when the researches of his colleague have been made known. With respect to Dr. Bryden's fundamental proposition, that the movements of epidemic cholera are independent alike of all terrestrial conditions and of human intercourse, although, doubtless, affected by these circumstances, we must wait for still wider and ampler evidence, especially that which will be afforded by synchronous statistical information from the other presidencies of India, including the civil population as well as the military, and the inmates of prisons, before we can rightly come to a sound judgment in the matter. The memorandum by the Army Sanitary Commission on his late report, contained in their last 'Blue Book,' well deserves his attentive consideration; it is an instructive and suggestive document, which, while it serves to stimulate him in the prosecution of his elaborate studies, should act as a useful check against the adoption of any premature conclusions.

In taking leave for the present from our Indian brethren, we would express our high opinion of the ability and zeal displayed in these reports. They have only resolutely to persevere in the systematic tabulation and comparison of well-ascertained facts, never seeking to build up until they have laid a sure and solid foundation, and to show the "hobbyism" and hasty theorising which have been so prevalent of late in this country, and we may confidently anticipate ere long some valuable contributions to our scientific knowledge of several of the most interesting forms of epidemic disease. It is abundantly obvious that our knowledge of cholera is still very incomplete and imperfect.

X.—Pathology of Aphasia.¹

THE name aphasia, whether the best which might have been devised or not, has established a place for itself in medical terminology. The introduction of a happy term has sometimes appeared to give an impulse to investigation, by serving as a centre round which observations may group themselves; but in the case of aphasia, this has probably had only a small share in attracting attention to the condition indicated by it, and the interest now so generally manifested in the questions arising out of loss of speech as compared with the neglect which the

¹ 'On Aphasia or Loss of Speech, and the Localisation of the Faculty of Language.' By FREDERIC BATEMAN, M.D., M.R.C.P.

earlier observations of Dax and Bouilland met with, is illustrative of the fact often exemplified in the history of science, that for a discovery to take root and fructify, there must be a certain advanced state of cognate knowledge. Very much still remains to be ascertained, both as to the anatomy and physiology of the nervous system, before the various forms of loss of language can be referred to anatomical or physiological conditions; and, in fact, much of the interest attaching to aphasia is excited by the hope and expectation that through it some additional clue may be obtained to the relation between the intellectual faculties and the structure of the brain, between mental processes and their material instrument. Already efforts in this direction have led to a closer analysis of the operations concerned in the expression of thoughts by words spoken or written; and this is a step in advance, whether or not it turns out eventually that a particular convolution of the brain is in special relation with language.

Whilst, however, in medicine a good name is of importance, there is always a tendency to elevate a descriptive term into a type, and to make of a symptom or group of symptoms a morbid entity, and thenceforth to look upon examples which present points of difference as deviations from a given typical disease; thus, under the same term, may be gathered cases which have only a superficial similarity. This tendency has never been more commonly exemplified than in the case of aphasia, and unless the most careful discrimination were exercised, it might become a source of confusion and error.

Dr. Batemen is the first English writer whose contributions to the subject of aphasia have attained the dimensions of a book. They appeared originally as a series of papers in the '*Journal of Mental Science*,' and on this account a certain much-needed allowance must be made in point of style and arrangement. Of the six chapters into which the book is divided, the two first contain brief notes of the contributions of previous writers; in the third, the author's own clinical experience is given; and the three remaining chapters are occupied with the discussion of the subject of loss of language from various points of view, one feature which comes out strongly everywhere being an attempt to overthrow Broca's well known hypothesis.

We shall dismiss the bibliography, as it is called, in a few words. The two chapters show an extended research, but the arrangement adopted renders the labour expended on them unavailable for any useful practical purpose. The writers are classified according to their nationality, and the principle on which examples have been selected for quotation has apparently been not so much the systematic illustration of the various

forms and degrees of impairment of language, as the collection of curious and so-called interesting cases. Many of the illustrative examples quoted, do not come within any definition of aphasia, if the term is to be defined at all; and, indeed, it is clear from the first that the author does not maintain a firm grasp even of the elementary distinction between defects of articulation and loss of speech as a form of intellectual expression. We have, for example, a case observed by Dr. Maty, introduced "as one of extreme interest from its exceptional character, being a case of impairment of articulate language from *disease of the spinal cord*," the said case being described as follows:—Six months after an injury, weakness of the left arm occurred, with difficulty of articulating certain words. Later the arm became withered, and the patient could scarcely utter a few words, and those monosyllables. All this time he could read and write. Can anything be more obvious than that this was simply an affection of articulation. Van der Kolk's hypothesis again, as to the relation between the olivary bodies and the complex movements of the tongue in articulation, is discussed as if it referred to a mental or intellectual factor in speech.

We have already said that a prominent point in Dr. Bateman's book is an examination of Broca's well known hypothesis, that the seat of the "faculty of articulate language"—we employ the term with a certain mental reservation—is the posterior part of the third left frontal convolution, and that lesion of this convolution is the cause of aphasia. It is natural that this should be so, and we propose to follow him in his criticism. It is essential, however, that Dr. Bateman, for the purpose of this discussion, should accept, at any rate for the time being, M. Broca's definition of the term aphasia. If Dr. Bateman means one thing and M. Broca another, Dr. Bateman's conclusions, from observation of what he calls aphasia, will not apply to M. Broca's conclusions from cases of aphasia as it is understood by him. Now, M. Broca has assigned to this term a certain fairly definite signification. The patient who is the subject of this affection has lost his speech wholly or partially, this loss not being due to paralysis of the tongue, or other parts concerned in articulation, or to general impairment of intelligence. Dr. Bateman, on the other hand, repudiates all definitions, and will not be fettered by any system of classification, but falls back upon what he calls the strictly etymological sense of the word which, according to him, is vague enough to embrace all cases in which speech is abolished or suppressed from whatever cause. We will not quarrel with him on a point of etymology, but we may be permitted to remark that the word aphasia was

proposed, and finally, after much debate, accepted precisely because it was believed to have a restricted signification, equivalent, or nearly so, to loss of speech as a mode of intellectual expression—to loss of utterance as distinguished from loss of articulation on the one hand, and from loss of comprehension on the other. Be this as it may, it is clearly necessary that we should know exactly what we are discussing when we speak of aphasia, and for this purpose we must analyse the operations concerned in speech or the faculty of language, as it is often called, a process we shall follow the more closely since it throws light on the mental operations generally.

M. Broca, as quoted by Dr. Bateman, says :

“There are several kinds of language; every system of signs which permits the expression of ideas in a manner more or less intelligible, more or less rapid, more or less complete, is a language in the general sense of the word; thus, speech, mimicry, dactylology, writing, both hieroglyphic and phonetic, are so many kinds of language. There is a general faculty of language, which presides over all these modes of expression of thought, and which may be defined,—the faculty of establishing a constant relation between an idea and a sign, be this sign a sound, a gesture, a figure, or a drawing of any kind. Moreover, each kind of language necessitates the play of certain organs of emission and reception. The organs of reception are at one time the ear, at another the eye, and sometimes the touch. As to the organs of emission, they are brought into play by voluntary muscles, such as those of the larynx, of the tongue, of the velum palati, of the face, of the upper limbs, &c. Every regular language then presupposes the integrity—1st, of a certain number of muscles, of motor nerves which supply them, and of that part of the nervous system from which these nerves arise. 2nd, of a certain external sensorial apparatus, of the sensitive nerve which supplies it, and of that part of the central nervous system with which this nerve is connected. 3rd, of that part of the brain which presides over the general faculty of language, such as it has just been defined. The absence or abolition of this faculty renders all kinds of language impossible.”

A more lucid analysis is that of Dr. William Ogle, “On Aphasia and Agraphia” (‘St. George’s Hospital Reports,’ vol. ii). Language, he says, is the communication of ideas by symbols. To *understand* speech, or articulate language, there are necessary—1. The ear. 2. The nerve to convey inward the auditory impressions. 3. A centre in which these impressions are converted into sensations. 4. Links from the sense-centre to the hemispherical grey substance, where the sensation recalls the idea. In aphasia all these processes are uninjured.

To *communicate* ideas by speech it is necessary—1. That ideas must evoke their appropriate symbols. Failure here is *amne-*

monic aphasia; the patient forgets words, uses wrong words in which the connection with the right ones is more or less traceable. 2. But a second memory is required—how to say words. Loss of this constitutes *atactic aphasia*. The patient in this case may have lost speech entirely, or he may retain a few words. It is no use helping him out; having forgotten how to say words, he cannot repeat them when they are spoken in his hearing. Broca's aphasia is the atactic form, and the faculty located by him in the third left frontal convolution is that of co-ordination of the movements by which words are produced. 3. A third requisite is the organ of speech, including the entire apparatus, nervous and muscular, for articulation.

These would be sufficient to indicate clearly what is meant by aphasia, but our object does not end here.

Dr. Bastian, for clinical purposes, divides cases in which language is lost into four classes, which he names—1. Amnesia. 2. Aphasia. 3. Aphemia. And 4. Agraphia.

In amnesia words are not revived in consciousness, and there is impairment in the process of thinking as well as of speaking; these defects of memory being either paralytic (words forgotten) or incoordinate (words misused). These cases are not properly included under the term aphasia at all, they belong to a thoroughly distinct class.

The other division he defines clinically as follows:

Aphasia, as including persons who can think but cannot speak or write.

Aphemia, including persons who can think and write but cannot speak.

Agraphia, including persons who can think and speak but cannot write. This last term was introduced by Dr. W. Ogle, to designate the loss of the art of writing, which commonly accompanies the loss of speech.

As classifications of functional affections of language there is no essential difference between the schemes of Dr. Ogle and Dr. Bastian, but it is an advantage to mark by the employment of different terms the distinction recognised by both, between the imperfect mental rehearsal of propositions and defects of utterance; and we should prefer to speak of amnesia and aphasia rather than of amnemonic and atactic aphasia, were it merely a question of nomenclature. The two classifications, however, are, as might be expected, the expression of different views of the physiology of language. Dr. Ogle defines the faculty located by Broca in the 3rd left frontal convolution, as that of co-ordination of the movements by which words are produced, and thinks it probable that memory for words is located in some contiguous convolution. We must, he con-

siders, deal with both faculties as one, the compound centre for both constituting the central organ of articulate speech. Within the same region again, will be the organ for written symbols; and possibly near these again, the organ for gesticulation, all these separate centres in close juxta-position forming the general organ of language.

Dr. Bastian's theory of the physiology of language, which forms a part of his general theory of the physiology of thinking, is, that impressions travel upwards from each sense-centre along a special and definite tract of white fibres, and impinge on the hemispherical surface grey matter at a special and definite part. Here the sensation gives rise to a perception, and the part where this occurs he names a "perceptive centre." We have thus an auditory perceptive centre, a visual, a tactual perceptive centre, &c. What the area occupied by these perceptive centres may be, and where situated—whether the visual and auditory perceptive centres are distinct, and more or less remote, or are interblended—we do not know. Words, as symbols to be used in thought, are revived in the auditory perceptive centre, and from this centre issue the fibres, along which the volitional impulse must travel, in order to translate thought into articulate speech, these fibres passing through the corpus striatum. In writing, the preliminary thought is carried on by words revived as auditory impressions, and these again revive, by association, visual impressions of the written symbols in the visual perceptive centre, from which the motor stimulus for their production starts on its downward course through the corpus striatum to the motor nerve nuclei.

In amnesia, then, in which there is inability to recall words, or an imperfect revival of words in the auditory perceptive centres, there must be an abnormal condition of the auditory perceptive centre, or some defect in that part of the cortical grey matter which has to do with the power of voluntary recall of past impressions; the seat of the morbid change is some part of the surface grey matter of the hemispheres. In aphasia the individual can understand and think, but cannot express himself by speech or writing; the defect is in the efferent tracts of fibres, along which proceed from the hemispherical grey matter the motor stimuli, which primarily incite the complex movement involved in speaking or writing. A lesion in one tract will constitute aphemia or agraphia; in aphasia there is lesion of both.

Dr. Bastian, not to speak of the revolution in the physiology of mind involved in the substitution of "revived perceptions" for "motor intuitions," which we cannot discuss here, has thus carried the analysis farther than Dr. Ogle; he has resolved the

general faculty of language into simpler physiological elements, and we do not need to speak of a central organ for this faculty, but instead of this we have auditory and visual perceptive centres, in which perceptions of spoken and written words are originally evolved, and in which they are re-awakened as often as is necessary by ideas, giving rise, or at least direction, to the volitional impulse which results in utterance. He has, moreover, rightly or wrongly, indicated with greater precision the seat of the lesion in amnesia and aphasia, placing the former in the grey substance, the latter in the white fibres. There is this, however, in common between Dr. Bastian and Dr. Ogle; both would lead us to look for the lesion giving rise to amnesia in the same portion of the hemisphere as that giving rise to aphasia. Dr. Ogle, after a most careful examination, agrees with Broca in the localisation of the organ for language. Dr. Bastian does not definitively commit himself to this; but if aphasia is due to rupture of certain efferent fibres, amnesia will be caused by damage to the cortical grey matter in which they rise; or if we accept for the moment Broca's hypothesis, amnesia would be due to lesion of the grey matter, aphasia to lesion of the white fibres of the third left frontal gyrus. Here, we think, is a weak point. We should not expect the perceptive centres to be themselves the downward starting point of motor impulses, but should look for distinct specialisation of the afferent and efferent departments here as in other parts of the nervous system, and just as we have in the spinal cord one cell-group for the posterior nerve roots, and another cell-group for the anterior nerve roots combined by cell-processes for reflex action, so we should suppose that the perceptive centres constituting the termini of the afferent system would be found at one part of the surface of the hemispheres, while the starting point of the efferent system would occupy another part, the two being associated by fibres. On this supposition, the third left frontal gyrus would, as Dr. Hughlings Jackson has put it, be simply the "way out" for language. Aphasia might arise from lesion of its surface grey matter, or by its isolation through damage to white fibres, and amnesic defects might be due to changes in the grey matter of convolutions more or less remote from it.

But we are altogether losing sight of our author; for this, however, he is himself to blame. He pronounces decidedly against Broca's hypothesis, and says that "of all the different theories which have been advanced, this least of all will stand the test of impartial investigation." We maintain, and we shall have to show, that whether this hypothesis be true or not, the conclusions against it drawn from his method of investigation are valueless. Since he throws together indiscriminately

all kinds and degrees of impairment of speech under the head of aphasia, and because some of these can be shown not to have been due to lesion of the third left frontal convolution, concludes that the relation said by Broca to exist betwixt this convolution and the faculty of language is disproved, we have been under the necessity of pointing out that considerable discrimination is called for. We have had, so to speak, to seek a canon by which cases may be tried, and in doing so we have availed ourselves of the labours of Drs. Ogle and Bastian, entering perhaps more fully into their speculations than was strictly necessary, because of the insight they afford into the physiology of mind. Our object has not been to give an account of the various important contributions to the questions arising out of aphasia, or we should have had to mention many names which will occur to our readers, and we could not venture at all on so fundamental a discussion as that raised by Dr. Macdonnell.

There is one writer on the subject, however, whose work cannot be passed over. Quite independently he had accumulated observations demonstrating the frequency of association of loss of speech with disease of the left cerebral hemisphere, and as he has more than any other physician illustrated by cases the various defects of speech, so has he set an example of exact investigation, and done much towards the elucidation of the physiology of language by original speculation. It is to be hoped that his collected and matured conclusions will shortly be given to the scientific world, and that he will not long be represented only by scattered fragments, often of opinions in course of evolution. We refer, of course, to Dr. Hughlings Jackson. We cannot here do more than give a brief summary of his analysis of language, which is made from a different point of view from those already given, and though not so available for our purpose, goes to the bottom of the question. He points out that language is of two kinds—1. Intellectual—the power to convey propositions; 2. Emotional—the ability to exhibit states of feeling. It is the first, and not the second, kind of language which suffers in disease; and this intellectual language suffers not only as regards words, but as regards writing and sign making. It is the power of intellectual expression by *movements* which suffers; those which are most special, as of speech, suffering most; the more general, such as those of simple sign-making, being the least affected. Emotional language is conserved throughout by modulations of voice, by smiles, by gesticulation. The speechlessness is not defect of voice, it is not paralysis of the articulatory muscles, and it is not due to fault in the receptive organs, but to interruption or derangement

of the sensori motor processes intervening between the receptive centres and the centres of emission.

As to the special nature of the defects of intellectual expression, we may have—(1) the sensori motor processes destroyed, when the subject will be speechless, or (2) they may be unstable, when there will be mistakes in words with perhaps a profusion of them. This is a distinction which goes deeper than that between amnemonic and atactic aphasia, or between amnesia and aphasia, and though coinciding with it up to a certain point is essentially different.

A radical distinction again indicated by Dr. Hughlings Jackson, on which much turns, is, that in intellectual language the movements are special and *acquired*; in emotional language they are *inherited*. All this leads up to an hypothesis which is advanced to explain the remarkable fact first brought into due prominence by him, that many persons absolutely incapable of intellectual language can yet, under emotion, utter distinctly and readily interjectional phrases; they are speechless, as he says, not wordless. The hypothesis alluded to is, that the left hemisphere takes the lead in voluntary, the right in automatic actions.

We are now prepared to examine the validity of Dr. Bateman's judgment on Broca's hypothesis. It is not necessary to follow his criticism on other hypotheses, which, without any sufficient reason, he calls popular. That of Van der Kolk relates merely to the mechanism of articulation, and its discussion in connection with aphasia is an instance of confusion of one factor of speech with another. As to those of Bouilland and Dax, which localise the faculty of language in the anterior cerebral lobes, and in the left hemisphere of the cerebrum respectively, they are superseded by that of Broca: it is obvious that if the third left frontal convolution is the seat of this faculty, they are both to a certain extent right, and the limitation of the lesion causing aphasia to a small part of the frontal lobe of the left hemisphere may explain cases which seemed inconsistent with hypotheses giving the organ of language a wider distribution, while it simplifies greatly the question to be settled.

Broca's hypothesis, then, rests on the following considerations:

1. In almost all the cases in which hemiplegia is associated with aphasia, the hemiplegia is on the right side, the cerebral lesion therefore on the left. The proportion of cases of right hemiplegia with aphasia to left hemiplegia with aphasia has been stated at 20 to 1; it is probably greater.

2. In some of the few recorded cases of left hemiplegia with aphasia, the patient has been left-handed, which fact, taken in connection with Broca's and Moxon's hypothesis of special

education of the left hemisphere in the right-handed and *vice versa*, as may of course be demanded by Broca, makes them more corroborative than twenty cases of right hemiplegia with aphasia, and indeed goes far to establish the unilateral localisation of the power of intellectual expression in the hemisphere which is most practised in volitional acts.

3. In a very large proportion of cases of aphasia, in which post-mortem examination has been made, the third frontal convolution, or the tract of fibres passing from it into the corpus striatum, has been involved in morbid change of some kind; in a certain number this convolution, or its efferent fibres, has been the only part of the brain affected.

4. In no case, says Dr. Ogle, after careful research, has the third left frontal convolution been found to be injured after death, without aphasia.

The points to be determined are, first, whether there are more cases of aphasia without disease of the third left frontal convolution than can be explained by the occasional substitution of the right for the left, and conversely, whether this convolution can be damaged without aphasia being induced, except in the case of a left-handed person. It is simply a question of evidence, only the witnesses must be competent, and, as we have said, the aphasia must be the condition understood by Broca and defined by Dr. Ogle as atactic aphasia, by Dr. Bastian as aphasia proper. We are here dealing, however, not with Broca's hypothesis, but with the grounds on which Dr. Bateman assumes to have overthrown it. He says evidence is daily accumulating which undermines it at every point. We have looked for it in vain. Strictly speaking, we need only concern ourselves with that he brings forward, but we may remark that at present any case militating against the hypothesis in question would, most probably, at once be made public, while cases confirmatory of it are likely to be held back. True, he has quoted cases of aphasia in which Broca's convolution was found intact after death. We admit that such cases have been observed; it has not yet been shown that they are sufficiently numerous to invalidate the enormous preponderance of cases in which it has been found altered. It is quite remarkable, too, how many of the exceptional cases break down under careful examination, and if the cases quoted by Dr. Bateman had been collected on rigidly statistical principles, instead of being taken at random, or chosen because exceptional in some way or other, a considerable number of them would have had to be eliminated. To attempt to sift and weigh the heterogeneous evidence, if it is intended as such, offered in the first two chapters, would be like beating the air. But Dr. Bateman has quoted a much needed

case of lesion of the third-left frontal convolution without aphasia, observed by Dr. Simpson of the Gloucester Asylum. Was it, however, we ask, the third-left frontal gyrus which was affected? The description of the lesion is as follows:—

“Both *orbital* divisions of the frontal lobes were indented from undue prominence of the upper walls of the orbits. On the left side, and implicating the *posterior part of the third or inferior frontal convolution*, a large depression existed, which appeared to be the remains of an apoplectic clot; it was of irregular shape, about an inch and three quarters in its antero-posterior, and an inch and a half in its transverse diameter; extending internally to within five lines of the olfactory bulb, and in front to within an inch of the anterior margin of the hemisphere; it was deepest in the centre, where it measured half an inch from the general line of surface.”

Surely this lesion was in the orbital lobule! The co-ordinates to which it is referred, and by which its situation is defined, are the olfactory bulb, which is in a different plane, and the anterior extremity of the hemisphere, the most remote point in the frontal lobe from the region of Broca. The third frontal convolution can scarcely have been implicated at all, it certainly cannot have been extensively involved. Another case is mentioned in a note, but on reading over this we do not feel confident that the gyrus in question was the seat of the lesion, which it is said involved the lower part of “all the frontal convolutions”—an expression we do not quite understand.

We naturally look, however, to the author's own cases for the foundation of his views, and it is stated that in five of them a careful autopsy was made, and in all, the frontal convolutions, which were examined with great care, were found quite healthy. We cannot be astonished that such experience as this should produce a strong bias, but its weight is remarkably diminished on close examination. The first case is entitled, “Aphasia of the atactic form, occurring as the earliest morbid symptom: some months later verbal amnesia: epileptiform convulsions: ultimately general paralysis.” The patient suddenly became speechless without paralysis. He began to recover a few words in three days, but it was a fortnight before any marked improvement was manifested, and four months later he spoke and wrote very imperfectly. Two years afterwards he seems to have been able to talk fairly well, and to carry on a conversation, but he was unable to remember names of things; that is, he is reported to have used such phrases as, “I know, but cannot say the word.” “To make up the fire,” &c. In the course of another year and a half, after a number of epileptiform attacks, he had become insane, imbecile and paralytic, and had completely lost speech, and he soon afterwards died. The post-mortem

appearances were congestion of the convex surface of the hemispheres with opacity of the arachnoid, both most marked on the left side. Softening in the middle of the left posterior lobe of the left hemisphere, and to a smaller extent at the corresponding point of the right hemisphere. Frontal convolutions, central ganglia, pons, &c., healthy. Arteries *atheromatous, left middle cerebral completely filled by a fibrinous clot*. Dr. Bateman thinks that the seat and source of the mischief was the posterior part of the hemisphere. Now, while admitting that opponents of M. Broca's hypothesis are quite justified in claiming this case as supporting their views, we think it is capable of another interpretation. There can be no doubt that the first event was plugging of the left middle cerebral artery, this causing the temporary aphasia. Later the blood-supply was partially restored, but not perfectly; whence the persistent imperfection of speech which, as has been stated, amounted for some time only to partial amnesia. But while the circulation was not adequate for perfect function, it sufficed to prevent marked structural lesion in some portion of the vascular area. The softening found in the central part of the hemisphere we should take to have been a later effect when the patient was insane and paralysed. Indeed, so completely is the third frontal convolution dependent on the middle central artery for its supply of blood that we should look upon a case in which this vessel was plugged without aphasia, transient or lasting, as far more conclusive against the functional attribute of this convolution than this case of Dr. Bateman's. It may be mentioned here that in one of Trousseau's cases of aphasia, without lesion of the third frontal gyrus, the middle cerebral artery was obliterated.

The second case is described as one of "Amnesic aphasia with right hemiplegia; softening of posterior part of left hemisphere; anterior lobes healthy." But we are bound to say, from the accounts given, that it was not aphasia at all; and whatever loss of memory for words was exhibited, was only part of general loss of mental power. The patient had become quiet and dull, then began to be confused in conversation, and did awkward things at table. When questioned by Dr. Bateman he got confused, and said he could not find words to describe his symptoms; yet what answers he made were given coherently, but in few words. The only instance quoted of mistake in words is, that he once said "poker" for "fire." While commenting on this case, Dr. Bateman expects that it will not be accepted by all as a case of aphasia; but later (p. 79), when it is wanted for the purpose of argument, it becomes "a marked instance of the amnesic form."

Case 3, again, is thus described:—Right and subsequently left hemiplegia, with lesion of speech. Frontal convolu-

tions sound. Here there was *temporary* loss of speech, with right hemiplegia. The patient afterwards spoke readily and unhesitatingly, but with a muffled tone, as if the mouth were full. And because this fugitive loss of speech is not associated with some permanent lesion of the 3rd left frontal convolution, the case is set down as evidence against the functional connection of this convolution with speech.

Case 4, again, denominated "aphasia, with right hemiplegia; no lesion of anterior lobes," is a still more remarkable instance of want of exactness. The patient, for many years occasionally the subject of mental excitement and delusions, is at length so ill as to be placed in a lunatic asylum.

"He now soon began to hesitate in his speech, and to give evidence of the loss of the memory of words; his power of writing also became impaired. Some months later he was suddenly attacked with convulsions, followed by right hemiplegia, with total loss of speech; and he died in a few days."

The post-mortem appearances were, thickening of the arachnoid, and congestion of the pia mater, with a deposit of bony matter in the centre of the middle fossa in the floor of the cranial cavity; no hæmorrhage or softening; nothing said about the condition of the vessels. It cannot be the growing imbecility of an insane man which is here meant by aphasia, and no careful observer would put down as an example of aphasia, and attempt to use in evidence for or against the localisation of the faculty of speech, a case in which death occurred within a few days of an attack of right hemiplegia, especially when accompanied with convulsions. It is well known that temporary loss of speech in such attacks is not uncommon; and until it was seen that this survived the shock of the attack, it could not be known whether it was the result of a momentary condition or of a distinct lesion, in which latter case only would it bear on the question. Here we have not even the assurance that the patient was not all the time in a state of stupor, for the passage quoted contains every word relating to his condition. It is to be remarked, also, that the hemiplegia, equally with the aphasia, is unexplained by any lesion. With respect to the three last cases, Dr. Bateman says that three negative cases, supported by post-mortem examination, go far to outweigh three hundred, where no autopsy was made. In point of fact, not one of them has the slightest bearing on the question. Another case, called left hemiplegia, with aphasia, has no greater importance. There was left hemiplegia, with considerable temporary embarrassment of speech; but at the end of a fortnight the patient spoke as well as usual. Two

months after the attack, he again lost the power of articulation, soon became comatose, and died in about twelve hours. It is clear that the aphasia, if such it was, and not a mere articulatory trouble, was not immediately dependent on the softening found in the left hemisphere, since the one passed off while the other remained.

The assault on Broca's hypothesis thus fails at all points; not, however, in consequence of its inherent strength, but because of the feebleness and want of method of the attack which it has been our business to expose. We cannot say that it is either shaken or in any degree established by the author's argument or our criticisms upon them, and when it comes to be finally judged, this book will not be an element in the adjudication. It is, indeed, strange to see so little appreciation of the facts of the case. Surprise is freely expressed at the *ne plus ultra* of localisation of a mental faculty, but it does not seem to have struck the author that the real matter for astonishment is, that loss of language should ever be produced by an unilateral lesion in the brain; nor does he seem to see that whether the lesion be constantly in the left hemisphere, or occasionally, also, in the right, an explanation is urgently required; or that a single case in which the only structural change associated with well-marked aphasia is limited to the 3rd left frontal convolution, constitutes a difficulty almost as great as Broca's hypothesis as a whole.

We enter now upon an entirely different part of our task. Dr. Bateman has, under the term aphasia, included all varieties of imperfections of speech; we must set aside our objections to the use of the word with this vague signification, and consider his book as a treatise on affections of language in general. It is unquestionably an excellent idea to take the faculty of language as a whole, and to seek in the various alterations induced by disease, whether fugitive or permanent, slight or extensive, accompanied or not by affection of the mind, for a more thorough comprehension of the whole of the processes concerned in speech; and it might be possible for an author to carry through this undertaking, so as to justify a complete change in the connotation of aphasia as a term, though we should in this case prefer Dr. Hughlings Jackson's plan of not using the term at all. But what careful discrimination, and what refined distinctions are required, we have already to some extent seen in the summaries of the thoughtful efforts of Dr. Ogle, Bastian, and Hughlings Jackson.

How, then, is this idea worked out in the book before us? We are compelled to say most imperfectly. The fourth chapter contains a very brief and inadequate analysis of language, and

a statement of the conditions necessary for articulate speech. These last are given as—1. The conception of an idea. 2. A connection between the idea conceived, and the conventional symbols constituting verbal language. 3. “The idea being conceived, and the verbal form found there must be integrity of the commissural fibres, and of the motor centres through which the volitional impulses operate in speech, and the muscles of phonation and articulation must be able to obey the mandates of the will.” 4. “It would seem that all these conditions may exist, and yet there may be aphasia or dysphasia.” “There lacks our fourth condition, the master mind, or what has lately been called *the power of co-ordination*.” We have to confess that we are here out of our depth, unless the author is. We do not know exactly who it is that has made the power of co-ordination synonymous with the master mind. Co-ordination, as we have understood it, is simply a convenient term for an orderly combination of ideas or movements or impressions; but the phrase seems to have taken possession of Dr. Bateman’s imagination.

The analysis of speech is followed by a mention of some of the classifications of the lesions of this faculty; and then comes the author’s own—not classification, but enumeration, of the different varieties. In this an attempt at arrangement is perceptible, though without the slightest reference to the analysis of language, or pretence to specify which of the conditions previously given as necessary for expression of ideas by spoken or written words is violated. As an aid to the comprehension of the relations of the various derangements of speech with each other, and with the functions of the different segments of the nervous system, the arrangement has much the same value as a botanical classification of plants into herbs, shrubs, and trees.

But even this self-imposed restriction has been too severe; for while speaking of some particular variety, a case will be given of an entirely different character; for example, variety No. 3 is characterised by loss of memory for substantives generally, apropos of which we have the remark, “Perhaps one of the most curious forms in which imperfection of speech shows itself, is where the defect is limited to some particular language.” And this remark is forthwith illustrated by cases. So, also, in variety No. 5, which comprises aphasic persons, who use certain stereotyped phrases in answer to every question, there is mentioned an old priest who, incapable of framing the simplest sentence, could repeat accurately long fables or passages from noted writings. A very curious example of confusion again, is to be found under variety 8, which we shall not stop to specify. Variety 10 is indicated by a special name, and is the only one

so distinguished; it is *aphasia spasmodica*. The term is new and original, and our readers will desire some information respecting it.

"Spasmodic mutism," then, "occurs in connection with hysteria and hypochondriasis, and may be of a more or less persistent character. Dr. Bright has recorded two cases in which the inability to speak coincided with hysterical trismus. A similar case was lately under my observation, the subject of it being a girl eleven years of age, who, after exposure to cold and damp, was brought to the hospital because the mother found she was unable to speak. On examining her it was seen that there was a forcible closure of the lower jaw, but the moment the mouth was pressed open she could speak as before. Dr. Willis mentions a curious case of this kind, which he calls 'paralysis spuria.'"

Why was not this hint taken, and the condition called "aphasia spuria"?

The account of the causes of aphasia is no better than the description of the varieties. Let us follow it as concisely as possible—1. Aphasia may be congenital, as in the deaf and dumb! and in some idiots. Not a word to show that the mutism of the deaf is a consequence of the absence of the sense of hearing, or a hint that another form of intellectual expression takes the place of vocal utterance. Class 2 of the causes comprises traumatic injuries. 3. Tumours. 4. Embolism. Then comes class 5. "It has been observed as a symptom of disease of the spinal cord." 6. "It may ensue as a nervous symptom"—(many persons under the influence of anger, joy, or excitement of any kind, are seized with a temporary incapacity to speak). 7. "The epileptic condition seems to be a frequent cause of aphasia." 8. "Aphasia is not an uncommon accompaniment of neuralgia and hysteria." 9. "Reflex action. Dr. Brown-Séquard expressed the opinion that aphasia was a reflex phenomenon." Sauvages speaks of *mutitas verminosa*. 10. "Several instances are on record in which loss of speech supervened on atmospheric changes, or on application of cold or heat to the head." 11. "Certain drugs would seem, in some instances, to suspend the power of speech." 12. "Septicæmia. Blood poisoning, whether from uræmia, alcoholism, gout or syphilis, is another frequent cause."

Comment is unnecessary. To look at this bare list, and see the overlapping, the recurrence of the same thing under various names, the indiscriminate mixing up of proximate and remote causes, it would seem that confusion could not be carried further; but the details show that it is possible. To make matters worse, one very common proximate cause is entirely omitted. There is no mention whatever of cerebral hæmorrhage.

One chance remains of bringing something like order out of the chaos of varieties and causes. There is a section headed "Diagnosis," and it is said that, as regards the various forms which this defect assumes, and the pathological conditions which give rise to them, the *differential diagnosis* becomes important. Differential diagnosis, however, resolves itself into a mention of aphonia, labio-glosso-laryngeal paralysis and simulation; and then we are told that, as we cannot afford, in an obscure subject like this, to dispense with any of the auxiliary aids to differential diagnosis, the thermometer, sphygmograph, and quantitative analysis of the urine may be of service. With respect to the two former, no original observations are offered; but it is the proper thing to speak of them. At various places the author speaks with a certain degree of complacency of his volumetric analysis of the urine; and in several of the cases observed by him, a single observation of the constituents of the urine is given. The relevance of the information obtained to any question requiring determination is not always obvious, and some of the results are curious; for example, a specimen of urine of sp. gr. 1020, contains 38 parts of solid matter per 1000; another, sp. gr. 1030, only 23.2; but work of this kind is to be encouraged, and it may be useful to point out that a single analysis, even if otherwise faultless, is of no great value, and that no conclusions whatever can be drawn from even a complete series of computations of the daily quantity of the urinary constituents, unless strict note be taken also of the daily food.

We have seen enough to make it clear that nothing is to be hoped for from this book towards the elucidation of the great problems connected with speech, nothing of value of any kind to be looked for, and we therefore discontinue our observations abruptly. There are sections on prognosis and treatment, and, oddly enough, the fact of prolonged speechlessness having been removed by free purgation and emetics, is gravely advanced as significant with reference to the question of localisation, and as a source of difficulty to those who adhere to the doctrine of separate and limited centre for speech. We have also considerations derived from human and comparative anatomy, physiology, anthropology, and a series of innocent questions as to possible electrical, thermal, or chemical causes for aphasia; but into these we do not enter. Let us give finally the author's conclusions—conclusions in which, as Johnson has it, nothing is concluded:

"1. That although something may be said in favour of each of the popular theories of the localisation of speech, still, so many exceptions to each of them have been recorded, that they will none of them bear the test of a disinterested and impartial scrutiny.

"2. That I by no means consider it proved that there is a cerebral centre for speech at all, and I would venture to suggest that speech, like the soul, may be something the comprehension of which is beyond the limit of our finite minds."

XI.—Holthouse on Hernia.¹

THERE is a popular sentiment, probably the result of the accumulated observation of ages, that the patient is safest in the hands of a doctor of the most experience in the particular disease under which the patient is labouring. Hence arises the modern tendency to resort to specialists in various maladies. This is a phenomenon by no means new in the world. The never-ending cycle of human conditions and events, influenced by similar motives, has led to the reproduction of a state of things well known to, and observed by, the writers of the ancient Roman empire, where the practice of medicine was conducted essentially upon free trade principles, and the cities, thronged by a hungry crowd of aspirants after fees, influenced by the same laws of competition which now prevail, and totally free from the salutary restraints of diploma examinations, were the seat of the minutest application of the principles of division of labour to such a degree as to extract from the orthodox mind of Galen the protest: "Unde mox si ita liceat loqui, plures etiam sint futuri medici, quam corporis particulæ; pro unâquâque enim affectione erit unus medicus" ('Galen, Tarasib.' Cap. xxiv). Oculists, aurists, throat doctors, stone cutters, rectum doctors, and fistula curers, and skin doctors, doctors for the old and young, herbalists and astrologers, were all there in swarms. And in ancient Rome, as in more modern times and countries, it appears that those who took up lines of practice which minister to the vanities and trifling complaints of the wealthier classes, were proportionately far better remunerated than those who had to deal with the more serious and fatal diseases, which always entail other expenses, solicit more strongly eleemosynary feelings, and necessitate more urgent attention in the poorer classes.

Another philosophic observation has been made, that in the conduct of affairs of all kinds, and in the practice of all professions, viz., that an attention too exclusively directed to one kind of practice induces a certain narrowness of view, and a

¹ *On Hernial and other Tumours of the Groin and its Neighbourhood, with Practical Remarks on the Radical Cure of Ruptures.* By CARSTEN HOLTHOUSE, F.R.C.S., Surgeon to Westminster Hospital, and Lecturer on Surgery.

On the proper Selection and Scientific Application of Trusses. By the Same Author. London, 1871.

want of many-sidedness in the intellect, which may lead to very serious mistakes in practice, and will tend to disqualify for the proper consideration of subjects which require a more extensive professional knowledge, and also for the estimation which results from general ability and eminence in all professional matters.

This narrowness of view reacts also very much upon experience itself. It has been smartly said, that a man whose mind is intent upon squaring the circle, will see a square in every circle at which he looks, and will sometimes find one. So a man who has a special vocation will find his speciality more or less in every disease, and give it an exaggerated importance. It is true he will sometimes be right, and when he finds the square, it has no doubt a very decided effect upon the veneration of observers who have not witnessed the numerous occasions in which the circle yielded no response.

There is, beyond doubt, experience and experience. There is experience in blunders and failures which are the consequences of a wrong theory, or a rigid routine and rule-of-thumb system in which a man grows old, and waxes so callous as he ages, as to attribute all the bad results to the *nature* of things, and not to his own infirmity; and there is an experience of the inconveniences of bad ways which stimulates a search after better, and their conversion into the beaten track. We think the better way is to be found by the cultivation of that sort of humility which leads to a truthful record of the cases that happen in a man's practice, and of the errors into which he may have been led, and to their use as guide posts, placed in such a manner as to enable every other man of experience to form his own conclusions upon safe grounds.

We are sorry to be obliged to say that many respectable authors have not quite succeeded in covering decorously the ruling object which undoubtedly instigates mainly to the production of the heaps of medical works in the present era.

It seems, at first sight, a pity that a profession in which so much sterling merit exists, and by which so much good work is done, is debarred by the inflexible laws of etiquette from the use of means to get at the attention of the public, which are open to, and unscrupulously used by, the quack and the pretender. But it is consoling to remember that this way is fouled by the very crowd which follows it to such an extent that the garments of the cleaner wayfarer must unavoidably be soiled by their contact, and that it is better, much better, considered in a merely expedient point of view, that it should be closed to the entrance of the legitimate profession.

Nevertheless, the temptation of seeing a short cut to the pockets of the public, successfully traversed by scores of pre-

tenders, sometimes influences the selection of a path which is dangerously near to the way of the wicked.

The works before us are not by any means chosen as an illustration of the bad effects of specialising the profession. In many of the cases related, the author seems to have afforded a straightforward and truthful record, though it were to be wished that more of them were such as had happened in his own practice; and fewer such as are already very easily accessible to every professional reader, and which usually happen to be those "cases in error" which are held up as an example or a warning of mistakes to be avoided by the reader.

In publications relating to subjects of so practical a nature as that of the works under review, it requires some measure of self-denial, and also of tact, to avoid giving them the appearance which is characteristic of the class of quackish puff which treads so closely upon the heels, and tries so hard to obtain a shred of the respectable mantle, or a ray of the reflected scientific light, which covers more or less completely the better portion of the list of medical advertisements in the columns of the 'Times.' We can hardly say that in the second of his two works under review, the author has been so successful as might have been wished in accomplishing this.

There is an interesting class of rupture cases, usually of the congenital variety, in which an effusion has taken place into the sac, and gives it a resemblance to hydrocele to an inexperienced observer. And even to those who have seen a good deal of rupture cases, there is a great danger of confounding them with those instances, not uncommon, where true hydrocele of the tunica vaginalis coexists with hernia, and is usually induced by a badly fitting truss, or by too great a pressure. Cases of the former kind are given by the author in those numbered 12, 18, 19 and 20. In number 12 a mistake in diagnosis by a rash general practitioner, led to the lamentable result of tapping and injecting the sac with tincture of iodine among the protruded bowels, and death from the consequent irritation and inflammation, which was entirely confined, however, to the contents of the injected sac. As a moral on the sad story, the author quotes some remarks from his lecture upon the subject, in which he mentions that Arnaud and Teale have recorded cases in which, from the distension of flatus and thinness of skin, the sac of a scrotal hernia was as translucent as that of an ordinary hydrocele. He proceeds to lay down the law that there is one infallible sign by which, as we understand him, a hydrocele may be distinguished from a hernia, and its diagnosis rendered certain, and that is *dulness on percussion*. It is rather surprising that it should not have occurred to him that in cases of effusion of

serum into a hernial sac, dulness of percussion as well as increased weight would be present in the lower part of a tumour just as certainly as translucency. And we would remind him of the extreme danger which follows attempts to lay down infallible single guides to those who have not had the benefit of experience in such matters, with a view of making a royal road to diagnosis. The temptation in teachers to do this is always great, and is continually leading their disciples to apply them blindly in ways which the greater experience of the master would make him "fear to tread."

Out of 53 cases given by the author we find no less than 22 taken from the practice of others, and mainly from sources already open to the public. Of these 13 may be classed as unsuccessful examples either of diagnosis or else in their results, and they are adduced, apparently, in the light of contrast; while 9 are fairly to be taken as cases which reflect credit upon the surgeon. Of the remaining 31 cases only 1 died (from ascites, after having the sac of the hernia tapped, in the supposition that the case was one of simple hydrocele).

The remaining 30 cases given and treated by the author were all such as would be considered successful in diagnosis and results. It is true also that they are nearly all cases which occur by the dozen in the practice of almost any hospital surgeon, and are habitually diagnosed and properly treated without especial record being considered necessary; but it may be said, on the other hand, that the commoner and more trivial cases are of more constant importance both to the profession and the public.

In No. 15 the author gives a case of congenital scrotal hernia, with orchitis and hydrocele, which was supposed to be a strangulated rupture requiring operation. Experienced caution induced delay in resorting to operation, since the symptoms of obstruction were not urgent. Ice was applied to the tumour, and the bowels were soon after spontaneously relieved. Tapping the hydrocele evacuated six ounces of fluid, and the testicle was subsequently successfully treated by friction with mercurial ointment. The previous history of this case illustrates a very common result, viz. an improper and badly-fitting truss inducing disease of the testis and cord by interference with the circulation. It also illustrates the danger which most impends, now that decisive rules of operating in doubtful cases are inculcated by teachers, of performing an operation not entirely free from danger in a case that really does not call for it. This has happened in several instances within the writer's knowledge.

Perhaps the most interesting cases in the volume are those which exemplify instances of tumours not really hernial, such

as cysts, undescended testis, glandular, fatty, and fibrous tumours, and even abscesses, have simulated, been mistaken for strangulated hernia and operated on, in consequence of the concurrent but independent existence of abdominal symptoms referable to the passage of gall-stones, renal calculi, and nephralgia. We can scarcely say, however, that any new light is thrown upon the diagnosis or treatment of these cases. In one case (No. 21) a suddenly occurring strangulation, in a young fellow of twenty years, caused by great muscular exertion, the treatment resorted to, after failure of the taxis, was a *hot linseed poultice* applied to the tumour, and the administration of a grain of opium three times. The result was the disappearance of the tumour next day. The reasons for the hot external application are not given; but it is not needed to go far for reasons directly against this practice, especially in cases of recent rupture, on the ground of the effect of hot applications in increasing the tension of the tumour by dilatation of the vessels, and the rarefaction and expansion of the contained gases. A hot poultice is an easy refuge when a man knows not what to do for a case of irreducible rupture, and we find it accordingly much resorted to under these circumstances, and it too frequently covers and masks the sure work of mortification beneath the surface. The opiates and rest had, no doubt, in the case given, much to do with the good result, in spite of the hot poultice.

In others of the cases, rather less appropriate for the treatment, perhaps, *ice* applied to the tumour seems to have been also successful, but with a much better "rationale." But ice is a remedy which cuts two ways, and is sufficiently powerful to do harm in cases not fitted for it, as when mortification is impending in the tumour. Sudden and recent cases, and old chronic cases of obstruction rather than of strangulation, are most benefited by its application, when applied early.

The last chapter of the book is devoted to the consideration of the *radical cure of hernia*. The author quotes the words of the late Sir W. Lawrence, to the effect that any cure for rupture, to be permanent, must act upon the tendinous sides of the canal and rings, and that a mere closing of the serous sac itself is not sufficient. He, however, scarcely affords an unqualified approval of this dictum, but is disposed to regard the seton operation, which acts on the sac only, "with more favour than is usually accorded to it."

The grounds for this opinion seem to lie within the compass of Case 52, in which Riggs's seton operation was done for an oblique inguinal hernia, which had descended into the upper part of the scrotum, and had existed three months. A year

after the operation the rupture returned suddenly, while making a violent muscular effort. As confirmatory of his opinion as to the efficacy of the seton operation, the author quotes also the post-mortem appearances in a case of Professor Carnochan, operated on after Riggs's method, on the 2nd May, 1857. The subject of it died of some pulmonary complaint, in September of the same year, and on opening the body the internal opening of the sac was found obliterated, and the upper part, though not the lower, of the inguinal canal closed.

In the operation which was performed, the skein of silk used being too large for the instrument, only one inch was introduced, which is said to explain the fact of only the upper part of the canal being obliterated. Fallacies may be concealed in this line of argument from a single case or two, both in respect to the accuracy of the original diagnosis, and in the short time, viz. a little over three months, which elapsed between the supposed cure and the post-mortem appearance. It is well enough known that many of the numerous cases in which the operation for closing the sac merely has been performed have been apparent cures for more than that time, during which the adhesion of the sac surfaces has been sufficient to restrain any protrusion which might be supposed to occur during a period of convalescence, when the patient would usually avoid any great muscular exertion, but which has not been finally effective in preventing the formation and descent of a fresh sac through the unobliterated canal.

From a paragraph which occurs in this part of the work we judge that the author, however, prefers, in cases of this kind, upon the whole, "the superior operation by the rectangular pins, which act both on the sac and the canal;" and we infer that he alludes to the operations and pins devised for children's cases, and small cases of this kind, by Professor Wood. A little further on, in the same chapter, a good many pages are occupied by a verbatim quotation from Mr. Wood's work '*On Rupture*,' describing the operation by wire, devised and practised by that surgeon, and the dressing and previous and subsequent treatment. From the prominence which the author gives to this quotation, we should infer that he is fully convinced of the great superiority of Wood's operation to any other of the very numerous plans that have been devised.

In common with every surgeon at all experienced in this line of surgery, the author has arrived at the conclusion that the risks of the operation are very small, and that its success, when proper cases are selected, is at least "quite as great as, if not more so than, in most operations of like magnitude, and that it is rare that the patient is not improved by it" (p. 165).

The author concludes by quoting the words of Mr. Spencer Wells, that—

“The relief of a strangulated hernia is justly regarded as one of the noblest triumphs of operative surgery. The surgeon saves the life of his patient without removing or deforming any part of the body. But the surgeon who cures hernia radically with certainty and safety is a greater public benefactor, as he not only relieves large numbers of his fellow-creatures from suffering and the inconvenience of wearing a truss, but he averts the danger of strangulation, to which they are continually exposed, in a greater or lesser degree, through every period of life.”

XII.—Recent Works on Syphilis.¹

Two years ago we reviewed a series of new works on venereal diseases, of which the appearance marked considerable progress in our knowledge of those affections, especially of constitutional syphilis. Before their publication the English language, rich in treatises on venereal diseases that had become classic by age, contained no work embodying the results of the labours of Ricord, Bassereau, Diday, Virchow, Zeissl, Lancereaux, and many who have gone deeply into the true pathology of venereal diseases. Not that English observers had been idle during the last thirty years; the contributions of Hutchinson, Wilks, and others, are every whit as valuable for a clear comprehension of syphilis as those of any Continental observer. But a systematic treatise, collecting the several discoveries of these workers, has for some time been supplied by more than one capable writer; nevertheless, some volumes have collected on our library table which need a passing notice at our hands.

We regret that the author of ‘*Syphilis Popularly Considered*’ has produced the work before us. The “popular” treatment of syphilis requires great caution, or misunderstanding is readily spread among readers not possessing scientific acquaintance with disease, if the description is loose, or if dogmatic opinions are enunciated on points of pathology on which no general agreement is held. A concise description of syphilis, giving each important point in the propagation and consequences of the disease,

¹ 1. *Syphilis Popularly Considered*. By JAMES GEORGE BEANEY, F.R.C.S. Melbourne, 1869. 8vo, pp. 297.

2. *The Pathology and Treatment of Venereal Diseases*. By F. J. BUMSTEAD, M.D. Third edition. Philadelphia, 1870. 8vo, pp. 699.

3. *Dactylitis Syphilitica; with Observations on Syphilitic Lesions of the Joints*. By R. W. TAYLOR, M.D. 8vo, pp. 30. (Reprinted from ‘*American Journal of Syphilography and Dermatology*’ for January, 1871.) New York.

and an impartial statement of the opinions held by the best observers on matters still in doubt, would probably be useful productions. But the volume in question hardly achieves this desideratum.

On the contrary, it is not free from errors of an important kind. The book mainly consists of a *réchauffé* of standard works on syphilis, of which some are quite within the range of the general reader's comprehension, and have the advantage of handling their subject in a proper scientific spirit. Mr. Beaney, while usually following the most approved authorities, has not successfully avoided introducing opinions not to be found in the authorities he cites, and which at the present day may almost be designated as popular fallacies no longer held by those well informed in such matters. For example, though excusing himself from describing primary syphilis, our author enumerates, as primary symptoms of syphilis, "balanitis, simple or ulcerative gonorrhœa, chancre, granular disease of the mouth of the womb, irritation of the testicle, prostate," &c. Similar errors scattered through an imperfect, and at times incorrect, description of constitutional syphilis render this book, we regret to say, an untrustworthy guide to a knowledge of that disease. It is better calculated to alarm and confuse the ignorant than to allay the apprehensions excited in the mind of the public by the puffing pamphlets of quacks, which the preface informs us was the object for producing the work under consideration. The book is illustrated by some very creditable chromo-lithographs, copies of several of the plates of Ricord and Cullerier, that in a really scientific treatise would have been well placed.

Into the third edition of his well-known and excellent work on venereal diseases Dr. Bumstead has introduced changes rendered necessary by the advance of our knowledge since the issue of the previous edition.

The book is very complete, including gonorrhœa and stricture, local venereal sores, and the varieties of constitutional syphilis. Dr. Bumstead, formerly held the local sore, for which he adopts Clere's term "chancroid," to be distinct from the initial lesion of syphilis, but he is now somewhat shaken from his previous belief in their totally distinct nature. He would seem inclined to put faith in Clere's theory that the virulently contagious principle of the local sore has degenerated from that of true syphilis—a singular doctrine, which has not yet found much favour with the medical profession. Dr. Bumstead has been induced to admit the possibility of this solution by the fact that matter of constitutional affections can be made auto-inoculable if the sore secreting it be properly irritated. But, as our author observes, it has yet to be proved that these suppurating ulcers,

derived from irritated syphilitic sores, have lost the power of communicating constitutional syphilis. Hitherto it has been believed that they retain that property.

Some observations communicated to Dr. Bumstead by Dr. Gjör, of Christiania, have induced the former gentleman to look favorably on the possibility of the local sore being descended from syphilis. It is difficult to see why the meagre report of these observations inserted in his text should be allowed much weight by so impartial and usually so acute an observer as Dr. Bumstead.

According to this report, Gjör had succeeded in making continuously auto-inoculable discharges, which he believed to have been originally the secretion of "mucous patches." From one of these artificial ulcers three patients suffering from non-syphilitic disease secretly inoculated themselves, and produced, not syphilis, but contagious sores that were readily propagated by inoculation. The patients were kept under observation for some time, but no symptom of syphilis appeared. These experiments are interesting, and they harmonise with similar observations of Bidentkap's, which do not appear to have met the attention of Dr. Bumstead; but they cannot be held sufficient to establish the doctrine that the soft sore is only the syphilitic sore without the power of infecting the constitution.

To settle this question we require unimpeachable evidence that the experiments were conducted with precautions to prevent error. In the absence of this proof we may fairly suspect their accuracy, knowing that Boeck himself, the chief of the Christiania experimental inoculators, when in England, habitually performed all his inoculations with the same lancet, only wiping it on a dry towel before passing from one patient to another, thus charging his instrument with matter from different sources. This, Dr. Bumstead remarks, was also Boeck's practice in New York, when conducting some experiments on syphilization at the Charity Hospital of that city. Hence, knowing that even Boeck must mingle the secretions of various kinds of sore, we can place no faith in this observation from Christiania until we have something better than the results of secret inoculations by patients on themselves, when no precautions could have been taken to prevent error.

In other respects Bumstead discusses disputed points in venereal pathology with great care and acumen. He has informed himself of the observations and opinions of the leading French authorities, being less familiar, indeed, sometimes apparently in ignorance of, the labours of German observers.

Speaking of the causes of gonorrhœa, Bumstead shows that urethritis must not be invariably set down to specific contagion.

He adopts Ricord's opinion that violent sexual irritation is an efficient cause of gonorrhœa, though contamination with contagious discharges is, no doubt, the prevailing immediate cause.

In the chapter on the use of mercury, which he upholds strongly for the earlier stages of syphilis, though writing with correctness and with tolerable completeness, it is to be regretted that the author has not availed himself of the interesting researches of Kussmaul and others into the vexed question of the influence of mercury in promoting the development of the late sequelæ of syphilis. These observers have made it clear that mercury has no influence in producing the ultimate tertiary forms of the disease. There is also a want of logical reasoning in the discussion on the modes of inheritance of syphilis. On p. 681 the author states boldly that the disease can be inherited from the father while the mother remains intact; yet immediately afterwards he proceeds to state and allow that the opinion that the mother never escapes if the child is affected, has much foundation in clinical experience, and mentions the law of Colles, who pointed out that a mother is never infected by suckling her syphilitic child, while foster-nurses are frequently so infected. This clinical fact Bumstead admits can only be explained by granting that the mother is already infected, either directly from the father or indirectly through the fœtus *in utero*. The latter hypothesis, it must be confessed, has next to no clinical foundation at all. In our opinion, it would have been wiser to have simply stated the facts that favour or oppose the supposition that semen unmingled with syphilitic secretions can give syphilis to either mother or child, instead of laying down a decision which the evidence scarcely warrants. On the whole, though the completeness, clearness of style, and practical directions contained in this manual, render it one of the best on its subject in our language, the author has failed to profit to the fullest extent by the literature which has appeared since the date of the second edition.

Dr. Taylor's essay on late syphilitic affections of the fingers and toes is a useful contribution to our knowledge of this rare form of disease, which has been generally overlooked by writers on syphilis. He has added to several published cases two that came under his own observation, and has constructed a short but valuable essay.

The disease consists of gummy infiltration of the subcutaneous connective tissue, the ligaments, periosteum, and bones of the phalanges. By this morbid process irregular enlargements are produced in both fingers and toes, sometimes forming rounded swellings of the bone and periosteum near the first phalangeal articulation, which strongly resemble enchondromatous tumours.

In other instances the enlargement spreads chiefly in the sheaths of the tendons and connective tissue, forming dull red brawny thickening of the whole digit. These affections yield readily to specific remedies. The essay, short as it is, contains all the cases hitherto recorded of this peculiar affection, and forms an important contribution to syphilitic pathology.

Bibliographical Record.

Nicholson's Manual of Zoology.—This little work is, perhaps, the most convenient manual of zoology yet published, and will, we doubt not, supersede the works of Milne-Edwards, Gervais, and Van der Hoeven in our schools. Conceived in a thoroughly philosophical spirit and written in an elegant style, it will serve as a basis for future instruction of a far more accurate character to that which we ourselves received in our younger days, when we, peradventure, had nothing more exact to refer to than Dallas's 'Animal Kingdom' or Swainson's useful but eccentric works. This book, as a whole, is remarkably deficient in those errors which are almost inseparable from all compilations. A few, however, exist, as *e.g.* that on page 331, where there is represented a diagram of the pectoral limb of the chimpanzee, in which only four fingers are represented, the thumb being apparently in a similar condition to that presented in *Ateles* or *Colobus*; and on page 467, where the fore part of the body in *Archæopteryx* appears cut off abruptly, and the peculiar structure of the claws on the fore wing, analogous to those in *Jacana*, are not exhibited. On page 402 it is stated that "the earliest unequivocal remains of Chelonians are in the Oolitic rocks (the *Chelonia planiceps* of the Portland stone)." We believe that it is now more than seven years since a species of turtle (*Chelys Blakei*, Mack.) was discovered in the underlying Stonesfield slate, which also contains numerous evidences of portions of the carapace of a land tortoise. A more serious error, however, occurs on page 562, where the antiquated opinion is repeated that *Plagiaulax* is "most nearly allied to the living kangaroo-rat (*Hyposiprymnus*) of Australia," and that *Thylacoleo* is a carnivorous and predaceous Marsupial. Now, whether we accept the views, which appear to us to be conclusive, of Professor Owen, or those which Professor Flower has promulgated, there can be little doubt that *Thylacoleo* and *Plagiaulax* must stand and fall together.

But whilst we consider that these and a few similar blemishes may

¹ *A Manual of Zoology for the Use of Students, with a General Introduction of the Principles of Zoology.* By H. ALLEYNE NICHOLSON, M.D., D.Sc., F.G.S. 1870.

detract from the value of Dr. Nicholson's work, there are other passages, far more in number, which stand in lucid contrast to anything we have previously met with in our elementary manuals of zoology. The diagrams of the attachment of the supescapula to the pectoral arch on p. 346 are extremely clear, and seem strongly opposed to Professor Humphry's theory of the arrangement and development of that part of the skeleton. The same distinctness may be predicated of the diagrams (we believe from Professor Huxley) of the extinct ganoid *Osteolepis*, as contrasted with its modern relative *Polypterus*. These convey, possibly, a clearer view of the structure of ganoid fishes than we remember to have seen in our palæontological elementary works.

It is perhaps to be regretted that in the classification of birds (p. 442) the order *Volitores*, which was so clearly established by Professor Owen in his (1858) lectures, has not been adopted, and that the serial arrangement of the orders of *Aves*, which places *Natatores* at one end of the series and *Archæopteryx* at the other, is not calculated to illustrate more distinctly the progression of the Avian class from the more general to the more specialised members. In fact, such a classification is scarcely superior to that of Linnæus, who ('Syst. Nat.,' ed. 13, pp. 346—358) regarded the goat-sucker and the tortoise as two genera next each other.

The version of Professor Owen's classification of Mammalia, given on p. 484, has the merit of being more comprehensible than those we have read in any previous compilation.

We may, indeed, say of Dr. Nicholson's work, that it has fulfilled nearly all the conditions of a perfect manual of zoology. It is alike lucid and comprehensive, and we have little doubt that it is destined to become *par excellence* the popular English manual of zoology of the future.

Physiological Essays.¹—These essays are reprinted from Indian medical journals, and are therefore practically new to most English readers. The first of them, "Drink Craving," appeared as long ago as 1863, and, although we cannot say that it contains much original matter, is a sound practical memoir. Amongst the remote causes of this affection our author places hygiene, climate and race, occupation, age, and mental constitution; whilst amongst the proximate causes he specially notices the habitual and excessive use of alcohol, hereditary transmission, and certain diseased states of the body. With regard to age as a cause of this disease, Dr. Bird remarks that he is "now of opinion that, by a natural law, the appetite for stimulants increases with puberty, remains strong through young

¹ *Physiological Essays: Drink Craving; Differences in Men; Idiosyncrasy; and the Origin of Disease.* By ROBERT BIRD, M.D., Bengal Army. Pp. 216. London, 1870.

and middle life, and declines in old age." He mainly bases this view, which, we fear, will not stand the test of statistics, on the case of an old gentleman who told him that, as a young man, he was devoted to the bottle and often got drunk, but that with his advancing years and grey hairs he had lost all taste for anything stronger than lemonade or ginger wine. Perhaps the author is not aware that in Scotland, and probably elsewhere, ginger wine has a very strong resemblance to flavoured whisky. We are sorry to find no original cases of hereditary transmission given. Under the next heading, "Disease of the Body," Dr. Bird contributes several original cases of considerable interest. The first is that of an infant drunkard. When nine months old she began to suffer from indigestion and diarrhoea, arising apparently from debility, and food of every sort induced immediate vomiting and purging. Ordinary remedies were utterly inefficient, and the mother predicted speedy death. Port wine was then tried, and from the first was not only tolerated by the bowels, but relished by the patient. From her mother's written history of the case we learn that "For two months she took nothing but port wine and arrowroot, and the four months following she lived solely on wine, brandy, and gin. Some days she drank one and a half bottles of port wine, and others not quite so much. She tired of that, and then took to brandy, and, generally speaking, used to drink one bottle a day; she also tired of that, and then drank at least a pint of Hollands gin a day. Food of any kind she would never touch during that time. The next three months she got on gradually to take her food, and is now as fine and stout a little girl as you could wish to see." We are happy to be enabled to state that the last we heard of this interesting child is that she is now fat, strong, and perfectly sober.

The last twenty pages of the essay are devoted to "Treatment." The author seems to place considerable confidence on the action of *Thymus serpyllum*, which was recommended by Linnæus, and has been more recently tried with great benefit by Dr. Salvadori. Dr. Bird has made use of the fluid extract of the plant, given in drachm doses every four hours; it seems to have a sudorific effect, to soothe and to induce sleep, and thus to have the tendency to ward off the threatened supervention of delirium tremens. It has not, however, in his opinion, the power of destroying confirmed drink-craving. Dr. Smirnoff is quoted, as recommending a strong infusion of the *Asarum Europæum*, or the irritant asarabaca mixed with valerian; having found these preparations valuable in Russia in counter-acting the invincible longings for alcohol which afflict the drunkard in the morning.

Considering the large number of chronic drunkards amongst our troops in the East, Dr. Bird recommends the establishment of a sanatorium for their reception and cure.

"If I am right (he observes), as I believe I am, that idleness, over-eating, too much sleep, and the relaxing climate of the plains in the rainy season, are the most common causes of drink-craving in India, then industrious occupation on a tea plantation situated on the slope of the Himalayas, at an altitude of 4000 or 5000 feet above the level of the sea, would, in addition to removing the patients from the influence of these causes, afford to their bodies an opportunity of returning to that state of health which is the only safeguard against the attacks of the malady from which they suffer" (p. 61).

Passing over the essay on "The Differences in Men," we arrive at one on "Idiosyncrasy," which was published in the 'Indian Annals of Medicine' for 1868. "When an unusual effect on any of our tissues is produced by any agent, then we are the subjects of an idiosyncrasy." How far this is a satisfactory definition we will not pause to consider, but it hardly serves to include all the cases he has collected. For example, he tells us that he attended for some years a family of three boys and two girls; and that when the younger girl was sick the others remained well, and that when she got well the others were sure to fall sick. This little girl, we are told, is idiosyncratic when compared with her brothers and her sister. What is the "agent" in this case that is referred to in his definition? He has collected and classified a good many cases of idiosyncrasy according to the tissue or organ specially concerned.

As a skin-idiosyncrasy he mentions the case of N. C—, who suffers from goose-skin when he listens to the sound which paper gives out when it is torn. Two cases of bladder-idiosyncrasy are recorded, in one of which a lad could not smell sweetbriar without being seized with an urgent desire to urinate; while in the other the patient, a little boy of three, could not sit down on silk without making water; dresses of other stuffs did not affect him, and he subsequently overcame the weakness. Passing over many other forms we come to idiosyncrasies in love, under which heading the author records the following case, which we should be inclined to regard as one of temporary insanity:

"— — was a young military man of fair family and abundant fortune, and passed to the world for a man of average abilities and good morals. He seemed to care little for the society of women, and it was therefore with no little concern that his relations discovered he had fallen violently in love with a woman who was twice his age, and who was so deficient in intelligence as to be considered an idiot. She also slobbered when she talked. So infatuated was the youth with this old woman, that he would sit by her for hours, purring over her and wiping the slobber from her chin. He recovered from his attack after two or three weeks' suffering, and he is now married in accordance with the wishes of his friends. The above case is

to be explained in the same way as a love of assafœtida and other stinks" (p. 175).

The analogy obviously does not hold, for the love of such substances as assafœtida is not transitory.

The last essay, on "The Origin of Disease," calls for no special remark.

Those of our readers who take an interest in rare and remarkable cases will find an abundant stock of them in this volume, which, if not very profound, affords plenty of information on the topics of which it treats, and is written in an easy, agreeable style.

Medicine, Disease, and Death.¹—Some two years ago Dr. Elam published in the 'Lancet' two papers, in which he endeavoured to prove three very startling propositions, namely—"1. That the average death-rate is slowly but constantly increasing. 2. That men die now at an earlier age than they did thirty years back. 3. That even those diseases which are best understood are increasing progressively in annual mortality unchecked by any resources of art."

Such heretical conclusions were not acceptable, and consequently two articles were elaborated to crush them. In this volume we have the original papers, the criticisms of the 'Lancet,' the rejoinder, and two other chapters, entitled respectively "The Aims of Modern Medicine" and "Eclectic Medicine." The three propositions we have quoted are based on an analysis of the death-returns from the beginning of the government registration up to the year 1866. By a peculiar grouping of the years, Dr. Elam shows that there has been an increased death-rate of about 1 in the 1000. There can be no doubt that the returns of the first few years are more or less inaccurate. The results of the registration in Scotland tend to prove this, and the Irish returns show it in a very marked degree. We think, therefore, that the returns for the first few years should have been omitted from the calculation. This plan has been adopted by the Registrar-General in his decennial returns, which commence with 1841, or three years after the beginning of a systematic registration. The mortality showed no sensible variation on the two decennials 1841 to 1850, 1851 to 1860; indeed, instead of the increase contended for by Dr. Elam, there was, if anything, a decrease in the registered mortality.

Conclusions which depend on the comparison of long period of the ten years are, at all events, less liable to be disturbed by accidental causes than those which rest on the comparison of much shorter periods, *e.g.* the mortality of 1863 to 1866, as selected by the author. Surely the results obtained by this kind of statistical conjuring

¹ *Medicine, Disease, and Death; being an Enquiry into the Progress of Medicine as a Practical Art.* By CHARLES ELAM, M.D., Author of 'A Physician's Problems,' &c. London, 1870.

afford no basis for the serious charge that the power of art over disease is diminished. That the death-rate has not grown less we quite admit, and possibly this may be considered by some to support Dr. Elam's view. In spite of all our efforts at sanitation, the mortality is as great now as it was thirty years ago; this is, no doubt, very disappointing, but is explicable on grounds other than those advanced by the writer. The enormous increase in the population of our great towns has taken place during the period that registration has been in force. In the thirty years 1831 to 1861 the population of eight only of our largest towns increased from two and a half millions to more than four and a half millions, while there was little or no increase in the country districts. Density of population, it must be admitted, does not conduce to health, and it must equally be allowed that it does favour immensely the spread of infectious diseases. Thus, we find the mortality of our urban population some 4 per 1000 more than that of the country generally, and nearly 7 per 1000 in excess of that of the country districts. The more reasonable hypothesis suggested by these figures, viz. that we have more disease to deal with, from the excessive growth of our town population, must, at all events, be disposed of before the degeneracy of physic can be admitted.

In Sweden, where registration has been in force for nearly a century longer than in England, the returns give us some reassuring facts. In that country, from 1755 to 1760, the deaths of infants under one year were 21 to 23 per cent. of all deaths, a century later they were only 16 per cent. In the second year of life the deaths were, 1755 to 1760, 6 to 7 per cent. of all deaths, a century later only 4 per cent. Surely these figures form as good a ground for asserting that medicine has increased her power over disease as Dr. Elam's do for affirming the contrary.

A very large proportion of the deaths in this country are deaths of children under five years of age, and it is to a lessening of these deaths that we must mainly look for a diminution in the death-rate. In the large town districts nearly one half the deaths are those of children under five years. Now, what figures have we to show that these deaths are due to the inefficacy of medicine? None. On the contrary, there are figures which show that the infantile mortality in our great centres of population is almost wholly independent of medical treatment. Not long ago a painstaking physician found out that, of 384 poor children brought to a children's hospital, nearly 200 had been previously without any medical aid whatever, and that on more than 150, druggists alone had tried their 'prentice hands. The results of such a state of things can scarcely be referred to the diminished power of medicine over disease!

Turning from the poor to the classes able to obtain medical aid, what do we find? The mortality among the children of peers under

five years of age, as quoted by the Registrar-General, is at the rate of 20·69 per 1000. Among the Society of Friends, the children's death-rate is 27·87. The children of the clergy die at the rate of 30·27. In the country districts the death-rate under five years is 40·34, while in town districts it reached 80·13 per 1000. These figures, serious as are some of the considerations which they suggest, do not, at all events, evoke the painful sense of impotence against disease which Dr. Elam imputes.

The evidence in favour of the second count in Dr. Elam's indictment is a comparison of the number of deaths above 85 for the three years 1847, 1858 and 1860. There is so much inaccuracy in the record of the age at death generally, as shown by the relatively greater numbers that die at the several decades, *e.g.* 60—70, that we must confess to having little faith in the value of this evidence. It is, at all events, too meagre to carry conviction. The enormous increase in the registered deaths from certain diseases is another well-known fact which is used in support of the charge against medicine. The great increase in the number of deaths from bronchitis is especially noteworthy. Dr. Parsons called attention to this in the 'Edinburgh Medical Journal' some years back, and, as far as we can recollect, referred it to the habits and mode of dress of recent times, an explanation probably as true as Dr. Elam's. Difference in registration, no doubt, accounts for much of the increase in several instances, especially as it is most marked in certain forms of lung disease and in heart diseases, in which our powers of diagnosis have improved year by year since the beginning of registration. To say that in all these cases our treatment has retrograded while our absolute knowledge of the diseases has vastly increased, is a paradox, which few will be inclined to accept. The assumption of an increase in the prevalence of these diseases is, we think, a more reasonable hypothesis, and one which should be disproved before resorting to the explanation of the author.

For our own part, we consider that the sanitation of the last twenty years has done very little to diminish our death-rate, and that preventive medicine has by no means kept pace with the increasing requirements of our growing population. This accusation, which is surely grave enough, Dr. Elam might have backed by a large amount of evidence, and we think he would have done more good by advancing it than he has done in attacking his own profession with such doubtful weapons as are used in this book. His dissatisfaction with the present state of therapeutics has, however, led him to try and show by arguments based on statistics how unsatisfactory is the present position of the art of healing. We heartily sympathise with his intentions, although we cannot congratulate him on the evidence he has brought forward in support of his case. The last two chapters of the book form a manly and able criticism of modern methods of treatment. Under

the glittering pseudonym of scientific therapeutics we have no hesitation in saying there has been concealed a vast amount of bad practice. It is quite honest to rebel against this sham; and in denouncing the vanity of "restorative" medicine and "expectancy," and other high-sounding emptinesses, which have lulled men to rest in a sense of false security, Dr. Elam has done us all a service. In the present day there is much need of an earnest medical iconoclast, who will dare to destroy in honest fury the idols we still worship. They are few in number, perhaps, but many still cling to them with an affection strong in proportion to their fewness, and we ought to be grateful to one who tries to inspire us with courage enough to face the obscure and unknown without such fictitious aid. Dr. Elam has done this, and we thank him.

Verity on Subject and Object.¹—The author of this treatise proceeds from a realistic point of view; that is, regarding subject and object as independent entities, he seeks to reconcile his tenets with the anatomical structure of the cerebro-spinal system of man. He holds firmly the belief that "object" is a real something external to us, and that our own mental states or activities are present to a superior psychical condition called *consciousness*. An explanation of the precise way in which he supposes the physical machinery of the sentient being, and the objects of the external world to fit those beliefs, forms the subject matter of this work. If this statement of the main idea of the book be correct (and we cannot, in the present state of our knowledge, suppose any author to maintain that he has argued up from the anatomy of the nervous system to a metaphysical theory), the remark is almost superfluous that these chapters convey nothing to those who have arrived at the conclusions that *real* externality cannot be proved, and that, psychologically, we cannot be, or very rarely are, in two states at once; and such conclusions have been rather widely accepted. Further, to those who still consider themselves as treading the paths of inquiry, the assurances of one who "belongs to those who rest in truth, and not in doubt," must be accompanied with a demonstration how it is possible to reach his secure premisses before the deductions he draws from these premisses can be accepted. Such a demonstration might have to consist in a reference to the works of previous thinkers; it is, however, given in no form, and both inquirers and opponents are left in the dark. So much for the way that the writer has made towards the end he appears to set before himself—"a complete philosophy in divinity, metaphysics, phrenology," and "physiology."

In the first chapter, on the "Origin and Causation of Consciousness," the author travels along the vagus nerve in search of the spot where

¹ *Subject and Object, as connected with our Double Brain; and a New Theory of Causation.* By R. VERITY. London, 1870. Pp. 86.

any disorder of it enters the domain of consciousness and becomes a sensation. He hits on the *optic thalami*, which he thereupon summarily erects into the organ in which "conscious life and personal sensation begin." Hereon follows a paragraph which, we hope, is not intended for a proof of this position. "In short (he writes) there is no part of the nervous system with which they (the *thalami*) are not either directly or indirectly connected." We need hardly say that it would not be easy to find a part of the nervous system which could not put in a claim on the same ground. These bodies, indeed, are two out of a number of centres (the corpora quadrigemina, corpora striata, floor of the fourth ventricle, &c.), which were surmised some time ago by Dr. Carpenter, on experimental and pathological grounds, to be probably connected with the step from merely nervous to nervo-mental activity. The selection of the *thalami optici* as alone concerned with this stage of nervous action, and the assumption of such certainty, in regard to their function, which is made to afford the basis of deductions, are entirely the author's, and rest on not a shadow of real evidence. For the fact that these ganglia appear more especially connected with the sensory tract of the cord, is of little value when we consider how our opinions of the seat of that sensory tract itself have been modified within recent years; and were it demonstrated, it would rather disprove than support the theory of *all* afferent currents becoming mental in the thalami.

But the author's next step consists in an assumption still more extraordinary, and again spoken of as a fact without the shadow of a proof being offered. It is this:—Since sensation is a form of consciousness, and since consciousness implies duality—viz. of subject and object—therefore the doubleness of the optic thalami corresponds to this duality, and therefore, in sensation, one optic thalamus corresponds to the sensation "*posited objectively*," and the other represents the subject! Some little confusion in the paragraph which announces this position inclined us at first to give the author the benefit of the doubt; but when we found further on that, "in recollecting and thinking, our mental system divides itself into two parts or terms, representative of what are called "Subject and Object," and these correspond to the two hemispheres of the brain" (the italics are ours), and, after a sentence or two, "one hemisphere (the right one) subserving the Subjective, and the other the Objective element of thought," we were compelled to believe that, both for the cerebral hemispheres and for the optic thalami, the fact of doubleness is interpreted as meaning that one organ contains, spread out as in a picture, the sensation or idea, while the other looks at it with the eye of '*consciousness*.' We may be pardoned for having immediately set about a search for other proofs that the book was a burlesque on Realism, which search was vain.

If a theory, to the conception of which the wish for it was so very evidently parent, be worth refutation, we would ask the propounder to step down from the thalami to the medulla or the spinal cord, and ask what means the doubleness there?—or, to run down the animal scale, to see that distribution of nervous matter follows the type of the body it is connected with, a law into the relations of which our knowledge of the time spent in a nervous journey between central organ and periphery gives us some insight?—or to consider whether the fact that stimuli which enter through two eyes, two ears, two nostrils, and so on, and act out upon two hands, two feet, two cheeks or eyebrows, are represented in their psychical phase by two similar sets of nervous matter, is such an extraordinary one as to require its being violently wrenched from its symmetry into distortion;—one set being assigned one function and the other a different and subservient one? Then what of the fact that lesion of a whole convoluted hemisphere has been followed by only a general weakening of mental life?

In his second chapter ("Origin and Causation of Perception"), the author, presupposing a *noumenon* and an *ego*, finds no difficulty in solving his problem; in the third, the proposition that effects are produced by "potentialities" present in the uniting causal elements, is either the old error of the verbal proposition, or the old doctrine of efficient causes revived, nevertheless it is set up as a "New Theory of Causation." The fourth reproduces the theological dualism of Thales; the fifth embodies an exposition of the views on the brain already referred to; and the sixth gives in the vaguest way some examples of a "New Method of Inquiry by Causation," which are selected from that class of events that suit the author's theory, the effect being but a shuffling of the elements of the cause.

Dyson Wood's 'Hamlet' Psychologically Viewed.¹—"On the occasion of an attempt to treat a subject of importance in a philosophical manner, the time, I think, is not inappropriate for an examination of the rather complicated and difficult questions—what is philosophy?—and what are its aims?"

This is the first sentence of Mr. W. Dyson Wood's pamphlet of twenty-seven pages, thirteen of the number are filled with a disquisition which may be called an *attempt* to answer the two questions propounded above.

After expressing an opinion, from which we think Mr. Wood will find many to dissent, that there is an innate tendency in each individual to claim the right of a philosophy for himself; or, in other words, that people generally do not attach the same meaning to the

¹ 'Hamlet,' from a Psychological Point of View. By W. DYSON WOOD, Assistant-Surgeon to the West Riding Prison at Wakefield. London.

term philosophy, we find our author venturing upon an explanation as to *his* view of the nature of philosophy, and, to use his own words, "its place in the realms of thought," at the same time claiming for *his* philosophy "a far higher position" "than any accorded by the views of others."

The process by which Mr. Wood proposes to build up this psychological philosophy is by "decomposing" the unintelligible chaos in which the mind is thrown when, at first sight, we look abroad into the world around us, into single facts, making a separation of these, not only in our own minds, but in nature; and then, having made this analysis, to "synthetically" combine them—*i. e.* mentally—until we can acquire the power of looking upon nature, not as a mass of confusion, but as one harmonious whole, in which "chance" has no place, but all is governed by a law or force supreme.

We do not purpose following Mr. Wood through his thirteen pages of not very clear argument, but shall refer at once, though in the briefest possible manner, to the ideal person he has chosen as his representative character.

And here, without the slightest wish to be hypercritical, we must, without hesitation, express our regret that the writer did not extend his pamphlet beyond the twenty-seven pages, because, what with the thirteen or fourteen pages of preliminary matter, and the four or five devoted to giving an outline of the play itself, the remaining pages have been by no means sufficient to enable the author to fully develop his views as to the mental attributes of the Prince of Denmark; indeed, so short of anything like a profound examination of the character of Hamlet have we found the work to be, that our thoughts have been irresistibly directed to the remembrance of those admirably written medical estimates of Shakespeare's sensitive and melancholy prince by Drs. Bucknill, Conolly, and others—writings in which the subtle workings of his imagined intellectual condition are exhaustively probed, and due justice done to the great dramatist for his penetrative observance of character, and the wonderful capability he possessed of depicting mental operations of all kinds, whether real or feigned.

It is, we think, almost an impossibility, if not quite an impossibility, to find in the writings of any author such master-pieces of *feigned* insanity as are drawn in '*Hamlet*,' and Edgar in '*King Lear*;' so successful and so perfect is the picture in each instance, that Drs. Bucknill and Tuke, in their '*Hand-Book of Psychology*,' describe them as complete to an extent that renders them difficult to be distinguished from the real, even by those skilled in the knowledge of mental alienation.

Of all writers, Dr. Bucknill has, perhaps, most thoroughly fathomed the depths of Hamlet's mind, and in the '*Psychology of*

Shakespeare,' by this writer, will be found so close and argumentative an examination of all that pertains to this most wonderful and intricate of Shakespeare's dramas that the task Mr. Wood has imposed upon himself must inevitably suffer by comparison; still, our author displays much thought and a just appreciation of many of the most salient points in Hamlet's character; and, if "Hamlet," from a Psychological Point of View,' falls short of what, from the title of the pamphlet, we were led to expect, it is, nevertheless, a commendable effort in a right direction, and it will be read by many, no doubt, with pleasure and profit. At the same time we may be permitted to say to our author, in all friendliness of spirit, that he would do well at present to remember the words of Polonius—"Give every man thine ear, but few thy voice. Take each man's answer, but reserve thy judgment."

'*Satires of Horace*,' by Dr. A. Wood.¹—The Greek and Latin classics seem to be in no imminent danger of being displaced from the prominent position they have so long occupied. The classic citadel has, indeed, sustained many a rude assault at the hands of unrelenting foes, but it has been no less gallantly defended by zealous and enthusiastic friends. And herein lies the secret of the successful defence which it has hitherto been able to make. There has been more heartiness in the defence than in the assault. While the one side has been content with cold and theoretical declamation, the other has given practical proof of zealous adherence and loyal devotion. The fierceness of the opposition, however, is now dying away. A reaction is beginning to set in. First one great name and then another appears before the public, with palpable proofs of the veneration in which the old classic authors continue to be held by men who have derived from the study of them the greatest profit in the cultivation of their mind and the training of their intellect. When such men as the late Lord Derby can find time, in the midst of a busy political life, to publish an elaborate translation of the 'Iliad' of Homer; when Mr. Trollope startles the world of novel readers, by showing that he has his 'Cæsar' at his finger-ends, and that he knows as much Latin as Bishop Proudie himself, or the Dean of Barchester; when Mr. Gladstone indulges in a little relaxation after the Herculean labours of a Parliamentary session by publishing a succession of literary gems from classic lore, the lovers of the old paths may well take heart and solace themselves with the conviction that their cause is not hopelessly lost, nor its defenders few and insignificant. Friends of "the old learning" stand up from every quarter, animated by no ignoble or professional zeal, but by pure, hearty, and sincere affection. How else can we account

¹ *The Satires of Horace, translated into English Metre.* By ANDREW WOOD, F.R.S.E., Fellow of the Royal College of Surgeons of Edinburgh, &c.

for the publication of such a work as a metrical translation of the 'Satires of Horace' by an eminent physician in large practice, who can find no more congenial relaxation after a hard day's work than by reproducing in English verse that portion of the works of an author in which he finds "writings" so much to his mind, "so genial, so versatile, so full of wit and humour and downright common-sense; so true to human nature—not merely Roman or Italian human nature—not merely the human nature of the time in which he lived, but universal human nature in all times"?

This is an admirable description of the writings of Horace which occurs in Dr. Wood's preface, where we also find such proofs of love for his author and such modest depreciation of himself that criticism is fairly disarmed, and we can only congratulate him on having achieved so successful a work in spite of the self-imposed metrical difficulty, added to the almost impossibility of doing full justice to such an author as Horace, and such a composition as the 'Satires.' If the vivacity, the elegance, the playfulness, the polished wit of the 'Odes,' present crucial difficulties to the honest and laborious translator, what must be said of the familiar, the colloquial, sarcastic, semi-poetical style of the 'Satires,' which run so naturally in a Latin, but so awkwardly and uncouthly in an English dress?

It must be borne in mind that Horace himself does not claim his 'Satires' to be regarded as true poetry. On the contrary, he thinks it necessary to remind his readers that he desires formally to exclude his name as a poetical writer

"From out the number of those men who may
Rightly upon the Poet's role abide;
'Tis not enough to give a rhyming termination
To lines, or write, like me, what's nearly conversation."

We think, therefore, that Dr. Wood has needlessly hampered himself by selecting the Spenserian stanza as the vehicle of his translation, although we are free to confess that he is often most happy and successful in the way in which he hits off the spirit of his author.

We have marked a few passages in which we note the true Horatian ring. Take, for example, a passage from the fourth Satire of the first book, in which Horace is moderately defending himself against the malice of his detractors:

"Take any one at random from the crowd:
One's to ambition bad, or avarice slave;
Another of his low amours is proud;
Another 'bout fine silver plate will rave:

* * * * *

"Nay, e'en through dangers headlong he will run,
Like dust before the whirlwind, almost mad
With fear lest he should lose what he has won,
Or lest he to his store should fail to add.

“ ’Tis men like these who verses fear, and shun
 With hatred Poets as a race that’s bad;
 ‘Fly them,’ say they, ‘put bay upon their horn,
 As with a dangerous ox you’d do, the passers-by to warn.’ ”

Or, again, in the third Satire of the second book, where the poet so complacently proves all the world to be mad, Dr. Wood has succeeded in imparting to his version no small portion of the liveliness and vigour which are so characteristic of the whole Satire. What could be a better rendering of—

“ Quid discrepat istis
 Qui nummos aurumque recondit, nescius uti
 Compositis,”

than this :

“ What difference, I ask, is there between
 Carrying to the deepest pit
 The wealth you have been fortunate to gain,
 And never making use of it? ”

We would only suggest the substitution of the word *consigning* for *carrying* in the second line, by which alteration we think both the rhythm and the sense of the passage would be improved. We notice an occasional tendency to diffuseness, which Dr. Wood would do well to repress when he next essays the difficult task of metrical translation. A single example will suffice to show our meaning. The well-known lines (‘Sat.’ ii, 2, 123—125)—

“ Post hoc ludus erat cuppâ potare magistrâ,
 Ac venerata Ceres ut culmo surgeret alto,
 Explicuit vino contractæ seria frontis,”

are thus expanded into the eight-line decasyllabic stanza :

“ When we had dined, ourselves then to amuse,
 Our cup unceremonious we quaff’d;
 We pray’d to Ceres that she’d not refuse
 To yield abundant harvests; and we laugh’d,
 Whilst with of rosy wine the temperate use—
 For ne’er did we exceed a sober draught—
 We smooth’d the wrinkles of corroding care,
 Which even frugal brows at times will wear.”

Now, to say nothing of the difficulty of giving a good idea in English verse of Horace’s ‘cuppâ magistrâ,’ a difficulty which Dr. Wood has scarcely mastered by the ingenious use of the word “unceremonious,” there is surely here too much verbiage—to be accounted for, perhaps, by the necessities of the metre, but a faulty verbiage notwithstanding. While, therefore, we cannot say that Dr. Wood has left us nothing to desire in his method of translating the Horatian Satires, we see in every page proofs of loving industry and intelligent appreciation of his author, which justify us in giving a hearty welcome to this latest attempt to introduce to English readers the light-hearted and quick-witted son of the Venusian freedman.

Valentin's Practical Chemistry.¹—We regard this introduction to qualitative analysis as the most valuable of all the works on this subject recently given to chemical students and chemical teachers. Intended primarily for the Royal College of Chemistry, it will be found most useful in all laboratories where anything like a complete course of practical chemistry is carried out. We say this in full view of the fact that the book is characterised throughout by a peculiarity in its chemical symbols which will not meet with general acceptance. We refer to the adoption of Dr. Frankland's irritating but ingenious method of notation—a complex method, difficult to learn, and involving a theory which, though plausible, is far from being proved. Yet as this is not the place nor the occasion to discuss the great question of the existence of bonds or vinculance in the elements, nor their modes of union, we may at once proceed to point out the aim of the volume before us, and the special merits which it appears to us to possess.

The first part of Mr. Valentin's book consists of twenty-two chapters, and occupies 128 pages; it is devoted to the manipulation and study of gases, and, indeed, of many typical and important elements and compounds. Some notion of this part of the volume may be gained by a reference to a few of the subjects discussed. In describing how to prepare hydrogen, oxygen, and certain gaseous oxides, the author takes occasion to give a very clear exposition of the laws of combination by volume, of what is meant by combination and decomposition, and by reduction and oxidation. The electrolysis of water is then noticed, and afterwards the atomic theory and the doctrine of constant combining proportions. After the description of sulphur we have an account at some length of its combinations with hydrogen and oxygen, and with metals, combinations of well-known importance in the processes of analysis. In fact, the description of the manipulation necessary to procure a substance is usually followed, not only by details regarding the properties of the substance, but also by such lessons on theoretical points of importance as it seems most appropriate to introduce. Two excellent chapters of this sort are numbered IX and X. They relate to the practical applications of the theory of constant combining proportions, and of the doctrine of quantivalence or vinculance of atoms. After the description of the acid-forming elements comes an account of salts and crystallization; just as the description of borax is followed by an account of the blow-pipe, its construction and uses, with details of blow-pipe fluxes, supports, and reactions. There is one admirable feature of this part of the book which must not be left unnoticed—we refer to the summaries at the end of many of the chapters, and the series of questions appended to all the rest.

¹ *A Laboratory Text-Book of Practical Chemistry*. By W. G. VALENTIN. Pp. x and 380. London, 1871.

The second part is more important in some respects than that just reviewed. It really represents in some sort the method of teaching qualitative chemical analysis which for a quarter of a century has been pursued in the best school of chemistry in the United Kingdom. Mr. Valentin, the Senior Demonstrator of the College of Chemistry, does not, indeed, claim for his work the merit of an abundance of striking novelties. But any one who has had a few years' experience in a laboratory will be able to appreciate the thoroughness, the usefulness, and the exactness of the system of instruction described on pp. 131 to 301. It would be foolish to attempt to reproduce its features here, for its excellence mainly depends upon small touches, upon the certainty of its processes, upon the anticipation of the thousand and one little difficulties of the student, upon the clearness, conciseness, and sufficiency of its descriptions. Besides the special emphasis laid upon those reactions which are to be used in the actual separations of metals and of acids, one noteworthy feature of Part II is the prominence given to commercial and medicinal preparations, and, above all, to important minerals. Usually books on chemistry, and chemists as well, neglect minerals wholly, as if the raw materials of our artificial products were of no moment, and their study could throw no light upon the formation and constitution of the preparations of the laboratory. The book ends with a series of qualitative methods in the form of tables, including a complete system for the preliminary examination of substances to be analysed, with general and group tables for the acids and bases. In four appendices are given accounts (rather defective) of reagents, tables of solubility, examples of analyses, and a few tables of metric weights and measures. A full index (twenty pages) enhances the value of the book.

A few slips occur here and there in the volume before us. We cannot at all understand why the author should go out of his way to affirm (p. 163) that "iron is one of the few metals which occur very abundantly in nature in the free state," when the exact contrary is the case. Nor do we think it advisable to recommend (p. 313) the student to use hydrochloric acid as a solvent for substances which have to be tested for phosphoric acid by ammonium molybdate. Nitric acid in the presence of ammonium nitrate is essential to a successful use of this most valuable reagent. But these and a few other errors and imperfections in the book do not really detract from its sterling qualities.

Dr. J. G. Richardson on *Microscopy*.¹—British observers will welcome this handbook of medical microscopy as a manual well suited for students. It has special claims for notice on account of

¹ *A Handbook of Medical Microscopy*. By JOSEPH G. RICHARDSON, M.D. Philadelphia. Pp. 333, figs. 30.

the very numerous references to the labours of living physiologists and physicians. Dr. Richardson has not only very judiciously chosen his directions for investigation from a great number of authorities, but has added many suggestions derived from his own experience. After referring to the different forms of microscopes used in America, he devotes a chapter, containing twenty-four pages, to the consideration of "instruments, apparatus, and manipulations." Then follows the subject of the "urine," divided into four chapters, extending over 100 pages. The arrangement adopted is that of Beale, from whose work many drawings have been copied; but the author also acknowledges the advantages he has derived from consulting the memoirs of Dickinson, Roberts, Grainger Stewart, Hughes Bennett, and many others well known in this country. The eighth chapter treats of pus, mucus, saliva, and milk. Next follow the blood, sputum in phthisis, under which head Dr. Fenwick's investigations and conclusions are very fully considered. Vomit and discharges of various kinds are followed by a chapter on animal and vegetable parasites. Some very good observations are made upon medico-legal investigation, including the study of blood-stains and the detection of spermatozoa, and the work concludes with some hints on the examination of morbid growths.

Most of the illustrations are copied from the well-known works to which we have already referred, and, upon the whole, are fairly executed, though they are very defective in point of number. In a book of this kind illustrations are really of more use to the student than description. At the same time it is but just to the author that we should state that he has carefully recorded in his work a brief outline of some of the most recent investigations, and in such a way as to excite the interest of the student and practitioner who may not be disposed to consult larger works.

Oldham and Balestra on Malaria.¹—The design of Dr. Oldham's work is to show that the main factor in the production of periodic or paroxysmal, *i.e.* of remittent and intermittent fevers, is the action of atmospheric vicissitudes on the system, and specially of a humid cold air at night following on exposure to high heat during the day, rather than of a specific aerial poison supposed to emanate from decaying vegetable matter in certain localities. That marshy districts are the most frequent *habitats* of the diseases in question is not, indeed, to be gainsaid; but then it is in such localities that the

¹ 1. *What is Malaria? and why is it most intense in Hot Climates?* An Inquiry into the Nature and Cause of the so-called Marsh Poison. By C. F. OLDHAM, M.D., Assistant-Surgeon, H.M. Indian Forces, &c. 8vo, pp. 186. London, 1871.

2. *Ricerche ed Esperimenti sulla Natura e Genesi del Miasma Palustre.* Dal Dr. PIETRO BALESTRA. Roma, 1868. Pp. 35.

Researches and Experiments on the Nature and Origin of Marsh Miasm.

meteorological phenomena alluded to are usually most conspicuous and most sensibly felt. Moreover, the geographical distribution of periodic fever is anything but limited to paludal districts, so that this fact alone may well excite a doubt as to the propriety of the common phrase of 'marsh poison' being used to designate their presumed morbid cause. Barren sandy flats in Holland, Spain, and other lands are it is well known, infested with what have been termed malarious fevers, although no marshy ground exists near them, and there be no trace of decaying vegetation on their surface. The writings of Pringle and Ferguson furnish numerous instances in proof of this. True it is that in many, if not in most, of the examples mentioned, there is reason to believe that the subsoil of the affected districts was more or less decidedly wet, and that water might be reached within a few feet of the surface. But this is far from being always the case in many dry and parched localities, in different countries, where fever abounds. Our author cites numerous proofs from India; among others that of the large military station of Mean Meer in the Punjab, and that of Tucobabad in Seinde, where fevers are more than ordinarily prevalent, although it would be difficult to discover in either place the supposed sources of malarial poisoning from any vegetable decay. Even dry rocky regions suffer terribly from malarious disease, as the medical history of Hong-Kong and Aden, as well as of a multitude of other similar instances referred to by Dr. Oldham, abundantly testifies.

"In most hot countries arid and stony places are considered by the natives to be feverish; and, in general, the more dry and barren (and therefore the hotter) the locality, the worse is its reputation. The people of Jhansi and of other towns in Central India attribute the fevers which so often prevail there to the influence of the bare rocks by which they are surrounded. Humboldt found a similar idea prevailing in South America amongst both the Indians and the Spaniards."

That great traveller mentions some villages near the cataracts of the Orinoco as being infested above all others in the district, although the surface of the country for miles round consisted of blocks of bare granite, utterly destitute of vegetable mould. The diurnal range of temperature was found to be excessive. Here is the clue, says our author, to the mystery of the supposed poisonous exhalations from the black-crust rock:

"The heat by day is very great, and so is the nightly fall of temperature, while the effect of the latter is intensified by the condensation of the enormous quantity of moisture in the atmosphere, which is alluded to by Humboldt as the probable cause of the fever."

In confirmation of the same general conclusion as to the unsatisfactoriness of the usually assigned originating cause of periodic

fevers, the attention of the reader is also drawn to the fact that in various regions, where marshes and marshy ground exist, these fevers are comparatively rare and unprevailing. Ireland, notwithstanding the large extent of its boggy lands, is by no means a country either of remittents or of agues. This "has long been a puzzle to writers on paludal poison." Of our settlement at Singapore, situated within a degree of the equator, and where all the conditions usually supposed to produce malaria—heat, moisture, salt marshes, rich soil and profuse vegetation—are in full force throughout the year, the official report states that "the station and district are healthy to a marked degree." And to take a similar instance in another part of the world, the valley of the great river Amazon, in about the same latitude as Singapore, is for the most part low and flat, covered with the densest tropical vegetation. Yet though the banks of neighbouring rivers under similar circumstances of moisture and vegetation abound with most deadly malaria, those of the Amazon are almost free from it. Humboldt alludes to this remarkable fact, and Bates, one of the most recent and observing travellers in those regions, confirms the statement in every respect in his interesting work entitled 'Naturalist on the Amazon.' The explanation given of the comparative immunity from malarial disease in the instances we have cited is to be found, according to our author, in the marked equability of diurnal and seasonal temperature enjoyed in the several localities mentioned.

For the detailed exposition of the peculiarities in respect of climate and meteorology in these localities, we must refer the reader to Dr. Oldham's work. He strengthens his position by referring to numerous examples of the innocuousness of works of irrigation in many districts of Hindostan, although in other districts opposite results have been experienced. Rice cultivation in Northern India, and many other countries, is well known to cause malarious disease, whereas in Southern India it is alleged to be not only harmless, "but in some instances, where the climate is equable and the heat is very great, the reduction of temperature produced by covering a large surface of country with water is considered to improve the health of the locality." The experience of General Cotton, of the Engineers, is that in Southern India no ill effects to health have been observed from irrigation. As to the supposition that malarial fevers are apt to be caused by drinking the water in marshy districts, an idea as old as the time of Hippocrates, our author maintains that no satisfactory evidence has ever been adduced in proof of it. At Amoy and Hong-Kong the same water was used by the shipping and the garrison; the latter were struck down with fever, the former escaped.

The conclusion of the whole matter he holds to be that "malaria, as a specific poison, produced by exhalations from decaying vegeta-

tion, does not exist," and that "malaria is chill." Although we are by no means prepared to accept this excluding and exclusive theory, we gladly admit that Dr. Oldham has worked out his views very ably and conscientiously. His work may be read with profit by all, and especially by the military and naval surgeon, who can, of course, best appreciate its numerous instructive data, and the soundness of the conclusions derived therefrom. The great practical lesson he draws from his researches, and one which is unquestionably of the highest hygienic importance, is that in malarious localities the utmost care should be taken to protect the body from cold. "The greater the degree of heat, and the longer and more continuous the exposure to it, the more vitally important does it become that even a slight degree of chill should be avoided." This precaution is always doubly needful towards evening and during the night. In the case of troops stationed in tropical countries, frequent temporary removals also to a cooler climate are among the most efficient means to counteract the evil effects of residence in unhealthy districts.

Dr. Balestra's experimental researches, an account of which was given at the recent international medical congress held in Florence, were conducted chiefly in the marshy districts around Ostia and in the Roman Campagna. Not only the water, but also the condensed atmospheric humidity, was examined microscopically with great care. His inquiries have led him to the conclusion that marsh miasm consists essentially of the spores of a specific *alga*, which he proposes to distinguish by the title of 'febrific,' or 'febbrigeno.' Dr. Salisbury of the United States had, two or three years previously, adopted a similar opinion. Dr. Balestra maintains that his *alga* differs altogether from that of his American confrère. His experiments have also convinced him that quinine is a powerful anti-miasmatic, by its direct action in destroying the morbid property of the microscopic spores.

In re Contagious Diseases Acts.¹—Really, after all the space we have devoted to the full and, as we endeavoured to make it, impartial discussion of the principles on which the Contagious Diseases Acts were based, and their operation in practice, we cannot undertake now a full review of these and a heap of other pamphlets upon the same subject which have reached us. We have, however, selected for a few words of comment two of the three subjoined brochures as pro-

¹ 1. *Recent Legislation on "Contagious Diseases" Considered, especially with Reference to the Army and Navy.* By FRANCIS CLOSE, D.D., Dean of Carlisle. Carlisle, 1870. Pp. 31.

2. *The Policy of the Contagious Diseases Acts, 1866 and 1869, tested by the Principles of Ethical and Political Science.* By SHELDON AMOS, M.A., of the Inner Temple, Barrister-at-Law, Professor of Jurisprudence, University College. London. Pp. 32.

3. *A Word on the 'Contagious Diseases Acts.'* By Surgeon-Major ATCHISON. London, 1871.

ceeding from men representing a class. The one regards the question at issue from the standpoint of a Churchman, educated and attached to that section of the establishment whose views upon social questions are the most restricted; the other from that of a lawyer who can imagine no interference with public licence justifiable so long as acts are not committed which the law distinctly recognises as crimes. Both writers have this in common, that they would leave the physical evils engendered and propagated by female prostitution to the slow working of that moral regeneration which nothing but the *millenium* will accomplish. As we perused Dean Close's pamphlet we were wondering what special qualification that distinguished, eloquent, and fashionable preacher possessed for his self-imposed task of instructing us as to the proper mode of dealing with the evils against which the Contagious Diseases Acts are levelled, or for constituting himself the critic of those Acts. We say most emphatically that this is not a subject to be written upon by any one who is not intimately conversant, by personal, close, and long-continued observation, of the habits and modes of life, the temptations and modes of thought, of the grade of society out of which prostitutes mostly spring. Is Dean Close such a man? Even if he be, it would have been just as well had he supplemented his experience by a careful study of the Acts and evidence he criticises. He would thus have been saved the trouble of a good deal of flighty *ad captandum* writing.

Professor Amos contrasts forcibly with the previous author in the sober and argumentative style of his pamphlet. He is an antagonist which the supporters of the Acts must meet upon his own selected ground. We ourselves believe that his opinions are capable of complete and satisfactory refutation. The line of argument which he adopts has nothing of novelty to recommend it. He endeavours to prove that the Contagious Diseases Acts are immoral, unconstitutional, and unjust. Should it become necessary for us to reopen the question in the Review, we shall not fail to give full consideration to the allegations of Mr. Amos.

Surgeon-Major Atchison addresses a few well-timed words to the ladies whose sympathy and aid have been invoked by the agitators against the Acts in question. He points out to them the real injury they are doing to society by the outcry they are either raising or encouraging. He cannot think that they really know the gravity of their opposition, and, on the part of ourselves and of many others, would willingly believe that they did not understand the subject at all. It is, perhaps, the worst feature of the opposition to the existent legislation that appeal is made so much to sentiment and so little to reason, and that its promoters have not hesitated to enlist upon their side the sex most easily moved by the former and least likely to subject their feelings to the control of the logic of facts.

Medical Diagnosis.¹—The first three works on Diagnosis of those

¹ 1. *A Manual of Medical Diagnosis; being an Analysis of the Signs and*

grouped together in writing this notice have already obtained the recognition of reviewers, and, what is of far more importance and satisfaction to the authors, have secured the good opinion of professional students, as witnessed by the several editions through which they have passed. They have severally been favorably noticed in this 'Review,' and now again, as revised editions, deserve commendation anew. Although they all deal with the same topic, each has its peculiarity, giving it special features to challenge the approbation of its readers. Dr. Barclay adopts especially the descriptive style, recounting signs and symptoms of disease in the manner usual in systematic treatises on medicine in general. Dr. Da Costa, on the other hand, brings more prominently into view the differential characters of diseases, and to this end introduces tabular sketches of the distinctive features of lesions likely to be confounded together. Lastly, Dr. Fenwick reverses as far as he can the plan of Dr. Barclay, and instead of narrating what ought to be found in a given disease, describes symptoms, and then tells their meaning, and the disease or diseases to which they point. At the same time, Dr. Fenwick does not undertake to give the mass of information contained in Dr. Barclay's work, relative to the methods of diagnosis, the lessons to be gathered, and the mistakes to be avoided in reading symptoms, and to many incidental truths in pathology. Indeed, this smallest of the three books in question is pre-eminently a book of instruction in diagnosis, and not a disquisition on the recognised features of diseases according to an accepted nosology. It is, likewise, of such dimensions that it may be readily carried about and be used by the student in his bedside explorations of the sick. And we hope that the favorable reception of his treatise will not induce Dr. Fenwick in any subsequent revisions of the text to enlarge its scope, and thereby sacrifice its present character as a manual. Corrections and emendations may be demanded, but we should deprecate its enlargement by additions.

Dr. Da Costa's work has the advantage over Dr. Barclay's, in the presence of some illustrative wood engravings; but of these it may at the same time be remarked, they elucidate little else than the most common-place facts, and are not likely to be valued by others than less advanced students in medicine. But the American work

Symptoms of Disease. By A. W. BARCLAY, M.D. Third Edition. London, 1870. Pp. 669.

2. *Medical Diagnosis, with Special Reference to Practical Medicine; a Guide to the Knowledge and Discrimination of Disease.* By J. M. DA COSTA, M.D. Illustrated with Engravings on Wood. Third Edition. Philadelphia, 1870. London. Pp. 844.

3. *The Student's Guide to Medical Diagnosis.* By SAMUEL FENWICK, M.D. Second Edition, revised and enlarged. London, 1871. Pp. 236.

4. *A Guide to the Physical Diagnosis of the Diseases of the Lungs and Heart, together with an Introduction to the Examination of the Urine.* By JAMES SAWYER, M.B., &c. London, 1870. Pp. 245.

5. *Auscultation and Percussion: together with the other Methods of Physical Examination of the Chest.* By SAMUEL GEE, M.D., &c. London, 1870. Pp. 299.

possesses another merit over and above its English rival, in its large clear type, making it more readable by all, and especially by those who have to give some consideration in the use of their eyes.

The two other works in the list are of cognate character, in being essentially occupied with the subject of the physical diagnosis of chest diseases. Dr. Sawyer, indeed, has some supplementary chapters on the examination of the urine, but the physical diagnosis of lung and heart disease constitutes the major portion of the book. As Dr. Sawyer describes himself as the Resident Physician at the Queen's Hospital, Birmingham, we presume him to be young in his profession, and are willing to accept this early attempt in medical literature on the footing he has placed it before us, as an honest endeavour to assist learners in the art of diagnosis, dedicated especially to students. As a contribution in furtherance of medical knowledge it could not sustain its claim, being nothing more than a digest of the best known facts respecting its subject matter. It conveys instruction in those facts in a lucid manner, and will no doubt be appreciated by many students. The mode in which it has been brought out reflects credit both upon printer and publisher.

Dr. Gee's treatise will satisfy the requirements of the most ardent and minute investigator in physical diagnosis. His instructions are minute and precise, and his examinations of the *rationale* of physical signs elaborate and lucid. The book is divided into two parts, of which the first is occupied with an account of the modes of physical examination and of the lessons to be derived therefrom. Accordingly, he treats of inspection, palpation, percussion, and auscultation generally, and adds an appendix containing fuller details respecting the exploration of particular parts and organs. In the second part he deals *seriatim* with the recognised lesions, first of the lungs, and secondly of the heart, pointing out their diagnostic features.

In the preface he expresses the "hope that the result of his labours will prove serviceable to those who wish to master the art of physical examination of the chest." This hope will assuredly be realised by all painstaking students of diagnosis who master the book. The ordinary run of students are not likely to appreciate it on account of its minute and elaborate details, yet the attempt to imbibe its teachings even partially would be a matter of good self-discipline to any learner, and would raise him nearer the desired condition of being a master in physical diagnosis.

Considering the attention bestowed upon the details of diagnosis in all works on medicine, the circumstance of the successful issue of several considerable works on that subject is somewhat remarkable. It at least exhibits much ardour on the part of practitioners and students in acquiring the art of recognising and discriminating disease; the matter of regret is, that the same ardour does not extend itself to researches into the treatment of disease, nor to an equal extent to investigations into the relations and the consequences

of morbid action. We need be thankful, however, that the discernment of disease, which is a long step towards its rational management, has of late years been so fully studied and placed on so secure a basis it now occupies.

Mr. J. H. James on Chloroform and Paracentesis of Bladder.¹—These two papers were written by Mr. James, of Exeter, in continuation of a series of similar works which he intended to publish as the results of his large experience. His executors, in the discharge of a debt of filial love, now present them to the profession in a separate form; but we do not think that their publication will do much to enhance Mr. James's reputation, although they contain some good and useful practical hints, mixed up with some speculative matter. Of the latter nature are the remarks about the use of pain in opposition to the employment of chloroform, but we cannot say that the utility of the anæsthetic is much disparaged, even if we admit Mr. James's opinions on the metaphysical value of pain to be sound. The remarks on paracentesis of the bladder above the pubes are interesting as personal records of experience in an operation which is not very frequently performed, but which is sometimes rendered necessary by peculiar circumstances. The nature of these circumstances is illustrated by some cases adduced by Mr. James, but many of them turned out unsuccessfully, owing to dangerous complications. Still, Mr. James has described very carefully the conditions which seem to demand the operation and the precautions necessary to be observed in its performance.

Dr. Murray on Snuff Taking.²—The prevention of bronchitis and consumption is a desideratum that no weak mortal can fail to long for. Therefore the subject matter of the book is calculated to be very "taking," although the taking of snuff be a dubiously convenient and desirable undertaking. However, Dr. Murray discourses so learnedly and agreeably, and so confidently, also, on the merits of snuff, that the reader will well-nigh confess to be almost persuaded to be a snuff-taker. He expresses thus his conviction, that "an habitual snuffer seldom, I had almost said never, dies of consumption;" and he further advocates snuffing as a preservative against infectious diseases.

In support of his proposition of the value of snuff as a prophylactic to chest disease, he lugs in the Registrar-General's reports; and like all other appellants to those elastic official documents, obtains a satisfactory decision. But not content with this, he collects in an appendix the favorable opinion of several correspondents, and altogether seems in a fair way of getting together a band of disciples intent on the cure of consumption through the nose.

¹ *Chloroform versus Pain, and Paracentesis of the Bladder above the Pubes.* By the late J. H. JAMES, F.R.C.S. Pp. 61. London, 1870.

² *Snuff-taking in Preventing Bronchitis, Consumption, &c.* By JOHN C. MURRAY, M.D. London, 1870. Pp. 64.

Original Communications.

I.—The Army in Relation to Public Health.—By CHARLES ALEXANDER GORDON, M.D., C.B., Deputy Inspector-General, Army Medical Department.

(Continued and concluded from vol. xlvii, page. 172.)

Food.—In considering briefly the subject of food in its bearings upon the matter now in hand, it seems convenient to discuss it in reference to its effects, 1st, upon the young men who offer themselves to the recruiting-sergeant; 2nd, as to its sufficiency and suitability for the recruit during the time he is undergoing drill and other training; 3rd, after he has become an efficient soldier, and is in the regular performance of his duties, whether in peace or in war.

1st. The conditions of diet that effect young men prior to enlistment do not of course influence the civil population directly through the army. They, however, have a very important although indirect bearing upon my subject, inasmuch that, the recruit being to all intents the raw material from which the soldier has to be manufactured by a complicated process, anything affecting the physical condition of the former must also affect the latter, and through the latter must concern the mass of the population in modes already more particularly indicated.

It is, I fear, a fact which cannot be denied, that a large proportion of the young men who from various circumstances enlist in the army have for a longer or shorter time been forced to subsist upon food deficient in the elements necessary for the supply of tissue. Few have had a sufficiency either of nitrogenous food in the form of *meat*, or of those farinaceous articles of diet which afford the amount of salts of lime and other mineral substances that are needed for the full development of the bones and dense parts of the body; thus the physical powers have never been developed to a degree capable of withstanding the effects of the first training to which the young man is subjected. The actual causes which bring about these results differ according to the county or district of the recruit, and are different in the rural districts from what they are in cities. For example, potatoes, which consist almost entirely of

starchy elements, are by themselves unsuited for strong muscular development; and it is proved in practice that young men who have been in a great measure fed upon these tubers break down in large numbers while being trained. In other districts a large portion of the food of the labouring class consists of doughy material and fruit, and recruits from them suffer in like manner to the former, although in a less degree, and among the younger classes in large towns it is almost needless to observe that much of their food, such as it is, consists of carbonaceous and hydro-carbonaceous elements, neither of which supply material for wear and tear of muscle.

2nd. *For recruits undergoing drill* and the other items of training necessary to fit them for their duties as soldiers, it must be obvious that the supply of alimentary material ought to be settled with reference to two considerations; namely, to compensate the system for the expenditure of tissue induced by their training; and still further to supply over and above the quantities required by that exhaustion, the amount requisite to complete the full development of all the bodily organs.

Under such conditions the daily quantity of actual nutriment allowed should be large, and I cannot help thinking that one meal per day should consist of oatmeal porridge and milk, as was the case for many years before the introduction of tea as a beverage. Meat in considerably larger quantity than is at present given should be allowed, nor ought it to be forgotten that peas, beans, and lentils are the most nitrogenous and at the same time among the most digestible of aliments. They were used extensively by the ancients, and they are really as appropriate now as they were formerly, and admit of ready use in a variety of forms.

3rd. *For the efficient soldier.*—Were it practicable to consider only the hygienic conditions of the soldier, without regarding the convenience of the army at large, it might be advisable to institute one scale of rations for recruits, a second for effective men in times of peace, a third adapted for times of war, a fourth for tropical countries or for use during hot weather, and a fifth for cold climates or seasons. Indeed, some writers go so far as to propose a system, or rather absence of system, under which soldiers should receive food according to their individual requirements and peculiarities. It must, of course, be self-evident that such measures would be utterly impracticable. Yet that much improvement might be effected is scarcely less apparent; for, while a few writers on the subject assert that the soldier is overfed, the larger number, and the most experienced, believe that he is, on the contrary, insufficiently fed; many, again, believe the system at present in operation to be a cause of disease and inefficiency in the army.

In considering the nature of food best suited for the soldier, it is

of course necessary to take into account the ordinary duties to be performed by him. These include drills, fatigues, guards, orderly duties, route marches, and others termed miscellaneous. Although none are of an arduous or severe nature, the continuance and frequent repetition of these duties produce gradual depression of the bodily powers, while their monotony and generally uninteresting character exciting no æsthetic stimulus by their performance, places them, in the estimation of the soldier, almost on a level with the hard labour he has to perform if sentenced to punishment for offences committed. Now, under such conditions it is fair to assert that the expenditure of tissue, although at no one time very great, is continuous, and thus in the long run far more exhausting than it would be in men liable to brief periods of great exertion, with intervals of rest between them; and hence that both the articles of diet and the time of issuing the meals ought to have reference to such circumstances.

With a view of indicating how far these objects are fulfilled, or left unfulfilled, by the existing system of feeding the soldier, I would indicate the composition of his ordinary rations, and times at which his meals are served up. He receives daily three quarters of a pound of meat with bone and a pound of bread from the State; vegetables, tea, or coffee, and such other little articles as he may desire, being purchased separately for his use. He breakfasts about 8 a.m., dines at 1 p.m., and at 5 o'clock has tea or coffee with bread, with such extras as his means afford, which are usually of a limited nature; thus, he has only one full meal a day, and between that of the evening and the light breakfast of tea or coffee and bread the following morning there is an interval of fifteen or sixteen hours, while a meal undergoes complete digestion in four to five hours. No doubt all these are supplemented more or less in the canteen; but beer and tobacco, every person knows, are more in vogue there than substances capable of supplying the waste of tissue and actual energy.

Perhaps the most ample scale of diet I have met with is that issued to the men of the 101st Regiment. It consists, in the first place, of the ordinary ration supplied by the commissariat, and for which the regulated sum of $4\frac{1}{2}d.$ per day is stopped from the pay of the men; in the second place, of articles obtained by private contract, or more frequently from the canteen, and for which $3\frac{1}{2}d.$ additional is stopped. These are shown below, namely:

Daily diet of a soldier in England.

Bread 1 lb., meat with bone $\frac{3}{4}$ lb.—Rations; $4\frac{1}{2}d.$ stopped.

Supplemented by bread $\frac{1}{2}$ lb., flour $\frac{1}{2}$ oz., barley $\frac{1}{2}$ oz., potatoes $1\frac{1}{4}$ lb., other vegetables 6 oz., sugar 2 oz., tea $\frac{1}{6}$ oz., coffee $\frac{1}{3}$ oz.,

mustard $\frac{1}{8}$ oz., pepper $\frac{1}{12}$ oz., salt $\frac{1}{2}$ oz., milk $\frac{1}{7}$ pt.— $3\frac{1}{2}$ *l.* stopped daily.

Total stoppage for food, 8*l.*

Total, solids, 67 oz. daily.

„ Liquids, 1 pt. coffee, 1 pt. tea daily; 1 pt. soup two or three times a week.

The amplitude of this scale is not questioned; on the contrary, it is freely allowed, but this very circumstance confirms the correctness of what is said in regard to the inadequacy of the Government scale, which is all that is given independently of private arrangements. Practical experience dictated the scale here alluded to, and I am forced to believe that, as Government requires soldiers to be maintained in every respect physically fit for the demands upon them, a scale of rations suited to that object should be issued to them, altogether irrespective of any private arrangements which may be established or not, according to the degree of zeal manifested by individual commanding officers.

Many are the proposals which have from time to time been brought forward to institute a scale of diet for the soldier that may be sufficient for his requirements under the varying conditions of his service; but circumstances other than of a hygienic nature have concurred to impede the adoption of anything approaching a scientific scale of diet. The point is really one of very great difficulty, yet it is to be hoped, for the sake of the soldier individually and of the army as a military force, that this problem will before long be solved.

Dress.—It is allowed on all sides that the unsuitable nature of the soldier's dress, with reference to the nature of work required of him, constitutes one of the most important causes of the great extent to which affections of the organs of circulation and respiration are found to prevail in the army. This conclusion was arrived at by the committee on the subject, dated 1868, as, indeed, it had been by the experience of medical officers long before; and it has been asserted that the comparative immunity from these diseases enjoyed by the French troops in Algeria was, in a great measure, the result of their being dressed loosely in that country—an explanation which is no more than half satisfactory, however, inasmuch as our own troops in India wear loosely made garments, and yet, as shown by statistics, suffer from these diseases to a very serious extent. So directly does constriction of the chest affect the action of the heart and blood-vessels that, as already observed, some medical officers believe the mere fact of placing an undressed soldier in the position of "attention" is sufficient to cause an audible bruit, at the same time that a diminution to the extent of twenty cubic inches takes place in the quantity of air inhaled at each respiration; when, there-

fore, in addition to a constrained attitude the soldier has to carry his kit, and be subjected to the compression caused by the necessary straps required to secure it upon him, it is easy to see in these circumstances sufficient cause, with or without others referred to, for the great prevalence of these diseases in the army.

We need scarcely dwell upon the palpable fact that pressure on the chest exerted by tight clothing prevents the free elimination of carbonic acid from the lungs. They become in consequence congested; the heart struggles to propel blood in large quantities through the lungs; the increased action of the heart, as of all other muscles, induces the formation of carbonic acid, requiring in its turn an increased quantity of blood to the organ itself, and, this not being forthcoming, the action of the heart speedily becomes irregular and weak. This, however, is no new discovery; it was pointed out by army medical officers many years ago that the system of dressing soldiers is directly productive of the diseases in question. For example, by Dr. Hunter, of the 2nd Regiment, so far back as 1836, and by others who, recognising the requirements of India, were instrumental in bringing about an improved style of costume in that country, more suited to the varying seasons than the thick woollens made to fit the body closely. This subject has been more fully entered into in my papers on "Army Sanitation in India," published in the 'Medical Times and Gazette' during 1868 and 1869.

It is generally considered that were the present dress of the soldier better adapted to the exertions and duties required of him, an important step would be attained towards the decrease of diseases of the respiratory and circulatory organs in the army. With the introduction of *running* drill and movements generally more rapid than were formerly practised, the necessity for loose clothing has become even more urgent than it has previously been. The dress should be loose; the hook and eye securing the collar at the neck should be abolished; all pressure from that part should be removed, and a loose neck-tie substituted for any stiff material for the neck. Much advantage is anticipated from the adoption of the *Norfolk* jacket. In order, as far as practicable, to guard against attacks of rheumatism, the soldiers should wear flannel shirts only. The use of leather stocks should be entirely abolished, and during the period of drill the neck of the tunic or other coat should be open.

The pack and accoutrements.—In considering the subject of dress some remarks were made relative to the influence exerted by the pack and accoutrements, which form an essential portion of the equipment of the soldier, upon his health. In alluding to the one it was not possible to avoid adverting to the other. With more particular reference to the pack, then, I would observe that the total weight a soldier had to carry, including the pack of the old pattern and its necessary accompaniments, amounted to 35 lb. 6½ oz. The pack

itself and its straps rested chiefly upon the clavicles, first and second ribs, and pectoral muscles, compressing, at the same time, the axillary artery and vein. It had to be carried at the greatest distance from the centre of gravity, which is situated at a point in the cavity of the abdomen, between the umbilicus and pubis, in a line from the centre of the third lumbar vertebra; and with the pack thus placed soldiers had to march and perform their greatest exertion, at the same time retaining the restrained and artificial positions demanded by military commanders. Under such circumstances it becomes easy to understand that very pernicious results to the health of the soldier were in this way produced. By reason of the very weight of the load he had to carry thus, badly packed, great fatigue and exhaustion were induced, demanding increased action of the organs of circulation and respiration, to supply the waste of tissue with fresh blood, and at the same time, the compression of the chest very materially interfered with the free action of both sets of organs. Such conditions sooner or later "told" even upon men who had arrived at maturity, but in the case of young lads, the bony frame of whose chest had not yet acquired solidity, the injury was more rapid and greater; nor must I omit to observe that, according to experiments instituted at Chatham with accoutrements of different nations, those of the British army were declared to produce the greatest amount of distress to the wearer.

It is quite true, as Dr. Lawson and others have pointed out, diseases of the heart and blood-vessels are most abundant at the Cape of Good Hope, a station at which soldiers did not, while on service, wear their knapsacks, and medical officers who have served much in India are aware that both pulmonic and cardiac affections are very frequent in that country. Notwithstanding the men are exempted from wearing their full accoutrements, special conditions are believed to exist at the Cape which give rise to the affections in question, and in India we discover in the intense and continuous heat, the alternations of climate, the malaria, and the confinement of soldiers to barracks during the greater part of the twenty-four hours, conditions more than simply sufficient to induce the prevalence of these affections there.

Having enumerated the above as important causes of inefficiency of the troops, the remedy seems plain. In fact, it will before long be demanded by the exigencies of the army, for with the late improvements in gunnery and in tactics the total abolition of the system of burthening troops with loads upon their backs when active movements are essentially necessary will soon follow. Great advantage is meantime anticipated from the introduction of the new and improved style of knapsack and accoutrements; but, as just observed, the system in accordance with which soldiers proceed on service laden with packs and other incumbrances must be changed. Battles

are said to be won more by the legs than by arms, and, considering the celerity of movements now rendered necessary, such changes must be near at hand.

Drills and exercises.—The life of a soldier has been described as one of indolence and excess, the indolence not only inducing vice, but also insufficient physical exercise. Regarding excess, it is not the mere amount of drill that causes inefficiency so much as the manner in which it is conducted. Thus, for example, with regard to recruits, no reference is made either to their physical condition while they are undergoing the ordeal of drill, nor to the nature of their previous employment; and it naturally follows that while one set of lads undergo their whole course of drill not only without injury, but with direct benefit, another set break down physically, and must be thrown back upon civil life. One set may have been accustomed to severe muscular exertion, but in unrestrained positions, yet they are suddenly called upon to assume a series of stiff and unaccustomed attitudes, demanding the action of an entirely new set of muscles, all of which are brought into rapid and severe action; thus, as may be supposed, not only debility, but actual *atrophy* of such muscles is not the uncommon consequence.

So direct is the influence exerted by such artificial attitudes upon the organs of circulation that, as already remarked, some medical officers assert as a fact that the circumstance of placing a recruit in a position of “attention” is of itself sufficient to induce a *bruit*, by reason of the pressure exerted by the scaleni muscles upon the subclavian and neighbouring vessels, the bruit ceasing on the attitude in question being relaxed. It is also distinctly asserted, and I believe with good reason, that affections of the heart and large vessels have numerically increased since the introduction of running drill, and, indeed, instances occur in most large garrisons of soldiers tracing their illness of that nature directly to this cause.

The late Dr. Hope mentioned the circumstance that the vessels of stags that had been run were frequently ossified more than those of animals that had been permitted to enjoy quieter lives, and it is fair to assume that a similar occurrence takes place in the case of men. At any rate, the statement is a suggestive one.

With the object of diminishing these evils as far as possible, exercises have of late been introduced, to afford the soldier intelligent and profitable occupation during the intervals between his necessary military engagements. It is recommended that drill should be made more varied than it is, that longer intervals for attendance at school than the present should be allowed, that occasional seasons of relaxation should be granted, trades taught, and that much of the first year of service should be spent in the regimental workshop.

Drill should commence after a course of gymnastics carefully conducted, and instead of the stiff attitude enforced by custom on soldiers they should be permitted to swing their arms, at the same time that, to enable them to carry their packs with the least amount of injury to themselves, a certain inclination of the body forward should be permitted. Recruits should not be kept needlessly long in over-strained positions, nor at drill too long at one time. The system of retaining constrained positions while marching ought to be relaxed, and marching in close order relaxed as much as possible. Lads of eighteen should be allowed at least two years to perfect their drill before absolutely joining the ranks. During the prevalence of epidemic disease, more especially abroad, drills should in part or altogether cease. One of the defects of infantry drill is that only certain sets of muscles are brought into play, and, according to the views of Dr. Parkes, it is not so well calculated to develop the physical powers as the occupation of the cavalry soldier, which is relieved by the variety of having to clean and attend to his horse. If running drill be a military necessity, we must accept a high ratio of these affections as a normal result, but the hours of drill should be modified.

Within the past year or so an important impetus has been given to the plan of employing soldiers in various kinds of work, and with manifest advantage, not only in regard to their own health, but in a pecuniary manner to the public. From the innovation at Aldershot, for example, there has been a saving of 10 per cent. in incidental repairs, and as much as 50 per cent. on some new works; at Woolwich 20 cent. on what is called incidentals, and 25 per cent. on new works; at Parkhurst a general saving of 30 per cent., and at the Curragh of 39 per cent. Experience has shown that health is best maintained, not only in the United Kingdom, but also in tropical countries, by continued occupation. As, then, we have seen how the condition of health of troops acts upon our population, and knowing as we do that poverty and idleness combined are perhaps the most prolific sources of physical and moral evils, is the question worthy of grave consideration, whether we should not train all soldiers to handicrafts, so that every description of work required for regiments should be executed by the men of the particular corps—barracks erected by them, furniture, clothing of various kinds made, and so on? Let each be paid according to his work, and let the proceeds of his labour go direct to his own pocket. Thus, while the lazy and improvident would probably remain in that condition, the willing, and more especially the married men, would be able to earn sufficient to maintain themselves and families in comparative comfort during the time they continue in the army, and on retiring into civil life they would carry with them in their handicrafts a means of independence so long as they chose to exercise them.

Gymnastics.—There is much reason to suggest that the course of

gymnastics to which young soldiers are subjected shall be conducted upon more physiologically correct principles than it now is. Dr. C. B. Williams observes that some most severe attacks of pneumonia in boys at school have occurred after violent and long-continued exertion, and he also mentions the derangements in the circulation caused by violent exertion. Other writers disapprove of the violent system of training and gymnastics to which recruits are subjected, and observe that, although athletics in moderation will no doubt develop the muscular system, when carried to excess they are positively injurious.

Night duties.—The frequent recurrence of night duties, together with the exposure and want of rest incidental to them, constitute a fertile cause of that prematurely old and decrepid appearance which characterises our soldiers, and more especially those of the line. It certainly cannot be said that there is much hardship in any one of the ordinary duties of a soldier; yet there is no question but that their continuous nature gradually expends and exhausts his muscular force, the periods of absolute rest being insufficient in length and frequency to enable the waning powers to recover their tone. There are other conditions connected with night duties which affect the men prejudicially, such as exposure to the elements, which induce attacks of rheumatism, affection of the pulmonary organs, &c., and thus lead to the non-efficiency of individuals, and in many cases to their discharge into the general population.

Intemperance.—Regarding the vice of intemperance as a cause of disease in the army, and thereby in civil life, I will not say much. That a considerable amount of sickness is indirectly as well as directly due to this vice is, alas! too true; and that men of such habits, on being discharged the service, add to the mass of disease, and often of crime also, is no less so. My belief, however, is that very exaggerated views prevail in regard to the actual extent of the vice of drunkenness in the army, an impression in some measure fostered by the very manner in which statistics of it are preserved. Thus, individual instances of intoxication are represented in numerical statements in such a manner as to give the impression that the figures really represent men. To render my meaning more clear, let us assume that in a regiment 800 strong there are forty men of drunken habits, that is, who are unable to resist the temptation to become intoxicated whenever the opportunity offers itself, the proportion they will bear to those of steady habits is as one to twenty. Let us suppose that each of those forty men is committed for this vice four times during one year; the case as shown in official documents will, in reality, give the impression that 160 men—that is, one fifth of the whole regiment—are drunkards. Then, again, is it not the case that a certain class of our countrymen, who themselves “live at home at ease” and discourse with solemnity on the transcendent wickedness of “the ribald soldiery,” evolve from their own inner

consciousness conditions of military life in which all sickness, all mortality, are due to vice and immorality.

I by no means desire to deny that much disease and other evils follow in the train of drunkenness, in the army as well as out of it; all I desire to urge is the simple fact that the evils which follow upon the vice are certainly not more prevalent in the army than they are in civil life, although I most willingly confess the need there is for improved conditions in both these respects. Whether to civilian or soldier, drunkenness brings a curse, and no measures should be omitted which seem calculated to lessen its evils.

According to one set of writers the use of alcohol tends to alter the molecular constitution of the muscular fibres of the heart, changing its various elements into fatty bodies, and causing deposit of fat between the muscular fibres and the external myolemma. Indulgence in the use of spirituous liquors thus predispose to these diseases even when the vice does not directly induce them, although it must be borne in mind that fatty degeneration is chiefly a disease of more advanced life, while such affections prevail for the most part in the earlier years of military service. It has been observed that alcohol chiefly exerts its effects upon the nervous system, and not upon the heart directly; nevertheless, as its continued use affects the liver and other internal organs, the circulation thus becomes deranged, and the heart ultimately suffers. Malt consumers in England and Germany are said to be more liable to gout and rheumatism than spirit drinkers. Delirium tremens is seldom followed by affections of the circulatory organs, and it has been considered that indulgence in spirits can only be looked upon as a secondary cause of heart affections.

With a view to diminish intemperance certain writers dwell upon the advantages to soldiers in India that would accrue from the more general use by them as beverages of tea and coffee, and of light wines; these should be reminded, however, that for years back every possible endeavour has been made, some with considerable success, to induce our troops to use these instead of the more potent drinks they are partial to indulge in. Some of the writers consulted object altogether to the use by soldiers of spirits, urging as their reasons that they do not impart strength, do not sustain under disease, nor protect against cold; others allow that they are of use in the treatment of zymotic diseases, but, inasmuch as the temperate are not exempt from these any more than the intemperate, they would sanction the moderate use of stimulants.

Tobacco.—Different views are expressed in regard to the operation of tobacco in inducing diseases of the heart and other organs of circulation; nor is it too much to assume that these views vary, according to the individual partiality, or otherwise, of the writers themselves for that narcotic. It is stated that there is no evidence

to show that the French or Germans are more subject to these diseases than are the English, and yet both of the former use more tobacco than the people of this country. Dr. Hassall is of opinion that in the plethoric and those with active digestion the use of tobacco is beneficial, but that it still further debilitates those who are already weak, and in this way may injure the soldier under such conditions; others assert, however, that these diseases are not more numerous among smokers than among non-smokers in the army. In persons who chew tobacco the evil effects which are produced by using the leaf, such as they are, are increased, but, on the other hand, the workers in tobacco manufacturers in France and elsewhere, although they acquire a peculiar expression and complexion of face, suffer no further injury. Dr. Chevers is of opinion that functional disorders are at times occasioned by the use of the leaf, although not in such abundance as to be of great importance; when also we observe that the young, in whom diseases of the heart are more frequent than in persons of advanced age, do not smoke more than the latter, it is evident that some other cause must be looked for.

There cannot, I think, be a question that to many persons the act of smoking tobacco is soothing and agreeable, filling a blank which probably could in no other way be so well supplied. At the same time it is equally clear that when indulged to excess, the practice induces various injurious effects, among which are impairment of the nervous power and dyspepsia, while it leads in the majority of instances to the vice of drunkenness, and thereby to many evils, physical and moral, recognised by all who take an interest in the well-being of the soldier.

Military punishments.—In considering the bearing of military punishments upon the health of the troops, and through them upon the public, I would observe that to a person unconnected with the army its whole code of regulations seems to consist of a series of arbitrary laws adapted for conditions which, looked at with a civilian's eye, are in many instances not of a criminal nature. The circumstance ought therefore to be borne in mind by those entrusted with the administration of such laws, that a soldier must have served in the ranks for a considerable time before he really understands the nature and bearing of the regulations. Many a young man errs in utter ignorance of the kind of penalty to which he subjects himself; many a one who is subjected to what appears to him an arbitrary punishment considers that he is improperly and unjustly treated, because he has neither had time to learn the code to which he is subject nor to have its provisions explained. The result is, he abandons himself to excess and crime, which sooner or later lead to his moral degradation and physical incapacity, first for the service, and then for earning his livelihood in civil life. May it not, there-

fore, be again suggested, as it was more than half a century ago by Dr. Robert Jackson, that officers should take as much pains to instruct the men under them in the meaning and purposes of the *Articles of War* as in their ordinary drill? It may in all fairness be said of the soldiers of the present day, quite as much, or more so, as of their predecessors, that there are few who are incorrigibly profligate; many who do err would cease to do so did they but receive that encouragement and consideration from those who are over them to which I have alluded. So far back as 1760 a code of instructions was published for the guidance of officers, from which I quote one paragraph that is now quite as applicable as it was then. "It is no part of duty, either as a soldier or a Christian, to add insults to confinement. Whatever orders you have to give," . . . "let it be done in an easy genteel manner."

In this place I will only refer specially to those punishments which involve increased labour and diminished food. In either case there is a disproportion between the expenditure of physical power and tissue and the material for the supply of both; consequently, direct injury to health results from frequent repetition or continued enforcement of such form of discipline. The system of reducing food either in quantity or quality as a part of military punishment is to be condemned, therefore, on the ground that, a certain amount being absolutely required to supply the ordinary demands of nutrition, diminution of that quantity leads to impaired force. This is injurious even if there be no punishment involving increased physical exertion, as, for example, shot drill, or even *extra* drill, but is trebly so if both are enforced, as is often the case when the system of the soldier is lowered by the effects of a protracted debauch.

The better to illustrate the conditions against which, on sanitary grounds, I would beseech the consideration of the authorities, I would subjoin the following bare statements, namely:

Table I. Showing the distance each man must march in moving shot while undergoing shot drill in military prisons.

Number of paces moved the shot.	Number of shot moved in a minute.	Number of paces per hour marched with a shot.	Number of paces per hour marched without a shot.	Whole distance marched in the hour.
3	6	1080	1080	2160
4	6	1440	1440	2880
5	4	1200	1200	2400
6	4	1440	1440	2880
5	5	1500	1500	3000
6	5	1800	1800	3600

Shot exercise may be made more severe either by increasing the weight of the shot to 32 lb., or by working as in (prison) exercise 4, the prisoners always having a shot in the hand when moving. The

description of shot used is, for first- and second-class prisoners, of 24 lb.; for third class, of 32 lb. weight. It must be obvious that a continuance of such punishment tells terribly upon the bodily powers, even were the man well fed. Let us, however, see of what his diet consists. This is shown as follows, namely :

Table II. Daily diet of military prisoners, home stations.

a.—Not engaged in hard labour.

Breakfast.—8 oz. oatmeal, $\frac{1}{2}$ pt. milk.

Dinner.—9 oz. Indian meal, $\frac{1}{2}$ pt. milk.

Supper.—8 oz. bread, $\frac{1}{2}$ pt. milk.

The following scale may be adopted after fifty-six days' imprisonment :

Breakfast.—10 oz. oatmeal.

Dinner.—12 oz. Indian meal.

Supper.—8 oz. bread. With $\frac{1}{2}$ pt. milk to each meal.

Prisoners of the first class, not engaged at hard labour shall be allowed a meat dinner as detailed below on Sundays.

b.—Engaged at hard labour, or after fifty-six days' imprisonment.

Breakfast.—8 oz. oatmeal, $\frac{1}{2}$ pt. milk.

Dinner.—8 oz. beef without bone before cooking, 2 lb. potatoes or 8 oz. bread, 1 pt. soup, thickened with 1 oz. oatmeal and 2 oz. vegetables, per man, seasoned with pepper and salt.

Supper.—8 oz. bread, $\frac{1}{2}$ pt. milk.

c.—If in solitary confinement for a prison offence, or placed on bread and water as a punishment.

1 lb. of bread, with such quantity of water for drinking as a prisoner may desire to have for the first three days of such imprisonment, which may, however, be extended to seven days, at the discretion of the visitor; and for the remaining period the same diet as for prisoners not engaged at hard labour. Daily.

A statement of the number of cases in which the extension beyond three days is awarded should be included in the medical officer's quarterly report.

The bread to be of the quality of ration bread.

On foreign stations the above scales of diet may also be used when deemed expedient; but if not, such diet only shall be used as may have had the sanction of the Secretary at War.

Every prisoner shall be allowed a meat dinner on Christmas-day.

The results of such a regimen, continued for any considerable length of time, and combined with the other conditions of discipline, such as enforced silence, and employment at uninteresting labour,

observed within military prisons, invariably show themselves. The individual loses power; he becomes not only weak bodily, but whatever mental energy he may have at one time possessed becomes gradually obliterated; hence the large number of wretched-looking men who frequent our military hospitals from those places, and who have to be "invalided" from these causes and thrown back upon civil life, injured in health, depressed in spirits, soured by their experiences of the army, and ready to raise their hand against society at large.

Notwithstanding what we have said regarding the evil effects of punishments which combine severe labour and diminished quantity of food, we learn that some writers are under the impression that soldiers may under certain conditions improve materially in health by such a regimen. It is asserted, for example, that the death-rate of soldiers in military prisons is smaller than it is among those in barracks in the proportion of two and a half per thousand, there being a similar difference in the ratio of sickness. I think, however, that in this case statistics alone are insufficient to give a correct view of the actual state of matters, inasmuch that men who while undergoing imprisonment become ill are at once sent to the nearest garrison hospital, when they are in considerable numbers brought forward as invalids and discharged the service. Then, again, a large proportion of men admitted into the military prisons are stronger and more healthy than the mass of their fellow-soldiers in barracks.

It is, I think, fair to assume that the recent introduction of a system of fines for certain offences, in supercession of such punishments as have been discussed, will be followed in due course by a diminution of the evils described. No doubt a further benefit would result were the province of principal medical officers of districts extended to military prisons in the same manner as they are to barracks and hospitals, in that case their control over the sanitation of the prisoners would of course become as direct as it now is over effective and sick troops. Another measure of a very desirable nature would be the careful medical examination of each man as he rejoins his regiment from prison, those only who are physically fit being permitted to return to the ranks, those who are not, being admitted into hospital for treatment, or to be "fed up" to the needed standard.

Malaria.—Among the many effects of malaria in a person saturated with that poison frequent attacks of palpitation and other indications of functional disorder of the heart are common, and these symptoms are known often to supervene upon attacks of intermittent and remittent fever. The weak, anæmic condition of men, and especially soldiers, who have resided for lengthened periods in tropical and malarious districts is easily recognised by the experienced eye, and the fact is well known that in many persons even who are fortunate enough to escape actual organic disease one of the effects of

malaria is to lower their condition of health for years, or perhaps always, thus reducing him more or less to a state of dependence, or to want in the event of his being dependent upon his own exertions for his living. And yet there are persons who know nothing of such countries who pretend to ignore the heat and malaria as causes of disease, and refer all illness among the troops to "drink." Were they in their own persons to spend some years amidst the hotbeds of disease in which our troops have to serve, they would doubtless learn by sad experience, perhaps in the shape of fever, or ague, or dysentery, to be charitable to others. They would, however, probably fear to write on the subject, for it is often the case that the more intimate the knowledge the greater the difficulty in expressing that knowledge on paper.

So numerous, indeed, are the manifestations of this poison in the system that there is hardly a form of disease that may not be in some way or other due to this cause, often reducing their subjects to a condition in which they are completely incapable of earning a livelihood in civil life, and thus driving them, sooner or later, into hospitals or poorhouses. Of such manifestations some of the most frequent are diseases of the liver, dysentery, and rheumatism; much of the affections enumerated as *neuralgia*, so often met with among persons who have been much abroad, are also due to the same cause, and all more or less render their subjects incapable of the exertion demanded in civil life, thus adding to the amount of physical inefficiency of the masses.

Hasty discharge of men from hospital.—It may safely be asserted that few men unacquainted with regimental routine are aware of the extent to which the health standard of soldiers is affected by the system, common in some corps, of discharging patients from hospital before complete recovery has taken place from the diseases for which they were sent in. Unhappily for the well-being of soldiers, it is customary in some corps to adopt measures of this kind so as to *keep down* the sick list, and accordingly men are sent to duty while yet weak; the result being relapses and increased severity of their original ailments until ultimately diseases which admitted of being completely cured, become confirmed, and their subjects, broken in health and incapable of earning their livelihood, are discharged the service. Sir James McGrigor many years ago saw this, and did his best to check the practice alluded to. A similar check ought now to be placed upon it.

Marriage in the army.—On the subject of marriage in the army in regard to its influence on public health, I can only say a few words. Six per cent. of our soldiers are by regulation permitted to marry on home service, and 12 per cent. in India and the colonies. It is well known, however, that in the United Kingdom the former proportion is exceeded to a material extent by clan-

destine unions, that is, marriages without the sanction of the regimental commanding officer; while in our more unhealthy possessions abroad, India, for example, the authorised proportion of marriages is never reached, for the simple reason that the supply of wives is by no means equal to the demand. Statistics indicate that, looking at marriage from a sanitary point of view, and that alone, the actual service performed by a married soldier is greater than that of the unmarried in the ratio of three to two; in other words, for every two years' actual service given to the State by a bachelor soldier, he who is married gives three, and, I may remark, an almost identical ratio is observable in the case of officers. To indicate the real bearing of these ratios, I must ask you to consider what they amount to in the aggregate in our army of 203,157 men, or even in that according to the estimates for the past year, namely 191,043. For the sake of comparison I take the first number, as my remarks are intended to apply to past and general conditions. Let us then assume that, balancing the army abroad and at home, there are throughout the whole 9 per cent. of the men married with leave, we have thus no fewer than 18,284 of this class. This is irrespective of the clandestine marriages to which allusion has already been made, and to which it is unsafe to apply statistics. According to the results already attained by statistics the above number of married soldiers perform as many years of continuous service as would 27,426 single men. Similar also would be the proportion of work in return for the number of days' pay: but the bearings of the difference do not stop here. When soldiers become inefficient, the part of the general work which each individual has to perform is thrown upon those who are for the time capable of performing it; thus if five men per cent. happen to be sick, under punishment, or otherwise non-effective, the actual work, and consequent wear and tear of physical power which would have been distributed over 100 soldiers must necessarily be borne by 95, and so on in a rapidly increasing ratio. Not only are soldiers thus the faster worn out and cast back upon civil life as invalids, but the greater becomes the drain on civil life to furnish recruits to take their places. The question has many important bearings in a purely military point of view, and from this it must chiefly be considered. I will not now enter upon the inquiry, however, it being sufficient for my present purpose to remind you that Cromwell's Ironsides were for the most part married men of the yeoman class.

With regard to public morals and public health, marriage in the army has a special importance. Constituted as this service is for the most part of men at that period of life when impulses are strongest, I need not enter into details of the numerous ways in which want of employment, and lack of self-control leads many into excesses which militate alike against morals and health. What may be the

ultimate result of the consideration now being given to the organization of the army in modifying the conditions of married soldiers, or in diminishing their numbers, I cannot say. This, however, I trust I may be permitted to observe, that while the effects of married life upon individual efficiency of soldiers is as I have stated, there is in the general condition of their wives and children very much deserving of attention with a view to amelioration. There is, for example, a general state of poverty, arising in part from the necessarily small rate of wages given to their husbands, and to the unproductiveness of the few kinds of labour in which they can at present take part. Considering how great are the gains to the public service which notwithstanding these disadvantages arise from the marriage of soldiers, we have every reason to conclude that they would be still greater were the condition improved. How this may best be affected is a problem to the solution of which much consideration is now being directed.

If we compare the extent to which marriage is permitted in the various armies of Europe, we find that, taking our own first, all regimental staff sergeants, and seven per cent. of rank and file, are allowed this privilege everywhere except in India, and there the proportion is increased to twelve per cent. In Spain, during the late *regime*, no man could marry until he had served six years, and the woman had to prove that she had sufficient means to maintain herself. In the Imperial Guard of France, the prohibition to marry is absolute; and in the line, not more than two or three soldiers per regiment of 3,000 strong have wives. In Sardinia, much the same principle prevails. In Austria, the privilege is confined as far as possible to non-commissioned officers. In Prussia, no soldier is permitted to marry during his first three years of service; and in Russia, on the other hand, marriage is both permitted and encouraged.

As regards the British Army, it is found that, inasmuch as love laughs at locksmiths, so Cupid, represented by the soldier, sets regulations and orders, in matters Hymeneal, at defiance. It accordingly happens that, besides those whose wives are what is called, "borne on the strength of the regiment," a number, varying according to circumstances, are "married without leave," and being so, enjoy none of the privileges, such as they are, that are permitted to the others. Taking both classes, statistics show that among the soldiers serving in the United Kingdom there are of those between twenty and twenty-five years of age 10·4 per cent. married; of those from twenty-five to thirty, 24·8 per cent.; from thirty to thirty-five, 37; and above forty, 48·3 per cent.—very large proportions, no doubt; but we must not forget the fact, that the proportion of soldiers in any regiment gradually decreases after thirty years of age. We, moreover, learn from statistics, that the circumstance of young men becoming soldiers itself exerts to a considerable extent a deter-

rent influence as regards marriage. Thus, among the civil population of England and Wales, the proportion of husbands of twenty to twenty-five years of age is 22·3 per cent.; twenty-five to thirty, 58·7; thirty to thirty-five, 75·5; and forty to forty-five, 82·6.

According to the statistics from which we quote, a very striking difference exists between the longevity of married men in civil life as compared with that of bachelors. For example, in Scotland, where alone the subject has been systematically examined, it has been observed that whereas the average age attained by married men is 59½ years, that of bachelors is only 40; in other words, after the age of twenty, married men are likely to live nineteen years and a half longer than bachelors. Such being the case in civil life, it is reasonable to conclude that similar results occur in the army. In a regiment, not only are vice and crime principally confined to single men, but it is a well-known fact that among them, the better disposed seek to marry with the desire to thus avoid temptation, which, in their single state, they find themselves unable to resist. Then, again, with regard to the public, let us but allude to the seduced girls, the amount of disease and suffering propagated among them and among the unhappy children to whom many give birth; let us, moreover, point to the vagabond population which owes its origin to the soldiery in our garrison towns, and we must acknowledge that the question of marriage of soldiers is in reality of importance, not alone as affecting the military classes, but in its bearing upon a large portion of the civil population.

As to the condition of the married soldier in the British Army at the present time, it has been well said that he cannot support himself, that he is to a certain extent a pauper, receiving relief which is never quite sufficient to meet his ever increasing wants; if he has a large family he is even insufficiently fed, and becomes actually physically inefficient as a soldier; if he be willing to work to increase his means he has but little chance of employment. It is true that the picture here drawn refers chiefly to the United Kingdom; yet what a melancholy state of matters does it display? And yet hard as are those conditions, they are far less injurious to health and efficiency than are those to which the bachelor soldier is exposed.

In India, the conditions of a married soldier, as well as of his wife and children, are very much more favorable than they are in this country, although even there room exists for improvement, more especially in extending to the families the privilege, now for the most part restricted to the single men, of being sent to Hill Sanatoria for the preservation or recovery of their health—a deprivation which of itself accounts for most of the greater mortality that yearly occurs among them as compared with the soldiers. It is this great mortality among them, combined with the comparatively unprolific marriages in that country, that render the actual number

of married soldiers in regiments some years in the country fewer than are permitted by regulations; for as the proportion of girls is exceedingly small who attain womanhood, and our soldiers have a natural repugnance against matrimonial alliances with the black inhabitants, the supply soon ceases to be equal to the demand, and men have to submit to enforced celibacy as well as to its various consequences. These remarks, it is true, refer to recent times. Some thirty or forty years ago matters were different. Soldiers in India then married native wives, sometimes from choice; others lived in a state of concubinage; drink of the most pernicious kind was nightly introduced by their wives or concubines, and the scenes, orgies, crime, and disease that were the consequences, was described at the time as something horrible. All that, however, is of the past. The conditions of the present day are happily very different, and it is to them that these remarks are intended to refer.

Marriage having thus been shown to conduce to longevity, to add to the military efficiency of soldiers, to the diminution of crime among them, as well as of vice, disease, and misery among the civil population, the questions naturally present themselves—by what means can it best be extended? and how are those drawbacks by which it is now beset to be remedied?

Most undoubtedly one of the greatest difficulties that beset the entire question of marriage among our soldiers arises from the conditions of military service, which are unlike those of any other army; thus, whereas no other troops are exposed to anything like the extent of foreign and tropical service that ours are, the remuneration given to our men for this almost perpetual banishment and exposure to pestilential climates is less than what can be earned by an ordinary field labourer at home; our system of so-called *voluntary* enlistment chiefly secures as recruits either the very lowest strata of society, the thoughtless, or the improvident and worthless, who are unfitted to “hold their own” in civil life. Such men have, as a rule, neither pride nor interest in the army further than that, while serving in it, their daily wants are supplied. They soon come to learn that even for the steady men, those who endeavour by regular attendance at the regimental school, to fit themselves for even the non-commissioned ranks, the chance of advancement is small; for promotion to officers, the chances are so much against them that they really can never be taken into account by a recruit of the ordinary class. Life in the barrack room is too often deprived of all pleasure by the continual supervision and interference of indiscreet corporals and sergeants; petty foibles are too often recorded as *crimes*; and the soldiers learn from their comrades, as they come to be discharged, that the rates of pensions awarded to them are not only very different from what they had been led to expect, but that they are inadequate, as a rule, to supply their most ordinary wants should health have

been lost. They see, in fact, that, constituted as the army is, there is little to bind them to it, but much to give rise to the belief that, however long they may serve, their actual or prospective conditions are but little, if at all, improved.

If, then, the social conditions of soldiers are to be in any considerable degree improved, the first and most important step to be taken is to render the army attractive to a better class than that which now supplies our recruits; the conditions of a soldier's life must be attractive to men of reputation and character, and to render exclusion therefrom a misfortune. Some of the means by which this end may be attained have recently been discussed by the author of a pamphlet on the state of the British Army in 1868, and of the practicability of the measure, not only without adding to, but actually with a decrease of, the army estimates, there is no question. Were service in the unhealthy colonies diminished, and the army opened up as a career for the yeoman classes, two of the most important measures to this end would have been attained.

Major Bannatyne believes that a great object would be gained if we could make the soldiers' marriage and return to civil life more certain and less distant than they are at present. He recommends that soldiers serving in the reserve force should be permitted to marry, and when they are called on permanent duty a daily actual allowance of 3*d.* for each woman, and 1½*d.* for each child under fourteen years of age, should be granted to their wives and families.

With reference to this proposition, it is to be observed that experience has yet to prove the readiness with which, did need arise, discharged soldiers would give up their homes, their little plots of ground, their wives and children, to take their place in the ranks on active service. History does not record instances where they have done so, and it is to be feared that were the experiment now tried with the present description of men who serve as soldiers, not only would their services be lost at the very time when they should be most efficient, but they could not afterwards be reckoned upon with confidence.

Military colonies, according to McCulloch, are generally admitted to have been a failure. The soldiers get attached to their farms and families; they become unwilling to leave them, and impatient of military restraint. Herr Von Haxthausen, in his comparison of the military colony system of Russia with that of Austria, says that in most places the idea of making both a soldier and a peasant out of a Russian was soon given up. Similar results followed a similar measure when some years ago it was tried in New Zealand and elsewhere. Pensioner colonists became dissatisfied with the very government to which they owed the domestic comfort and independence that they enjoyed; with comparative wealth came to them a voice and influence in the community of which they formed a part, and

these they were not slow to employ against their benefactors. Hence the system was abandoned. Nor is it likely to be entertained again.

“Brown Bess” asks the question, Why not make the permission to marry one chief advantage of the re-engagement of soldiers, the soldier devoting his whole future life in consideration of the provision made for his wife and family? This proposition implies that Government should make direct provision for the wives and children of its soldiers, thus acting towards them in an exceptional and very different manner from what it does towards any other free classes of her Majesty’s subjects. The plan is therefore not likely to be adopted; yet it may become matter for consideration how far permission to marry, with good quarters for the wife and children, might not be held out as an inducement for deserving men who have completed their first term of service to re-engage. But here two difficulties meet us, the first being in reference to the reckless or thoughtless, who, as young soldiers, marry without permission, and whose families are usually in a state of absolute destitution; the other as to how far the extra expense thus incurred by encouraging old soldiers to continue in the army would be repaid to the State by their greater efficiency. There is every reason to believe that it would not, and that therefore arrangements directed to only one class would end in failure.

What, then, seems the most practicable way of meeting this important and difficult question? The improvement in the prospects and position of the soldier to which allusion has already been made, being once effected, it would then become practicable to make terms with candidates for enlistment. Our army, if numerically smaller than at present, would gain in respect to quality. It might then be made a condition of engagement that the recruit should for the first five years of his service remain unmarried on pain of dismissal, if serving in the United Kingdom, or any of our possessions having a temperate climate, admitting of manual labour. It is moreover to be observed that among other shocks given to many of our cherished notions, the campaigns of America and Prussia have shown how unnecessary is the extent of continuous drill to which it has been the custom to subject soldiers. Would it not therefore be possible to permit soldiers who are able to work at handicrafts to do so for their own individual profit; those who desire to learn being trained to particular trades and the proceeds placed to their credit in the regimental savings bank? They would thus, in the event of marriage, be in a position to earn the means of maintaining their wives and children in comparative comfort; it being understood that most, if not all the work now executed by contractors should be given to regiments. Another very important means to the same end would be that of reducing to a minimum the moves of regiments, which at

present are such a source of expense to the soldier; and another would be that of making better provision than has hitherto been made for the families of soldiers, while the latter are employed in active service. The direct outlay occasioned would be inconsiderable, while the increased efficiency of the soldiers would, it is calculated, far more than repay the State for the first expense. Sickness and crime being diminished, so would the expense and loss of service incurred by curing the one and punishing the other. Soldiers would feel that their labour was remunerative to themselves; and in course of time their sons would enter the regimental school, as at present, but with some knowledge of mechanical art, such as would occupy their spare time and enable them to earn a fund for their future requirements. The question is doubtless of far too great importance to be exhaustively discussed within the present space; all, therefore, that can be attempted is to direct attention to some of its more prominent phases.

The general conditions of military service.—The works, essays, and reports on army hygiene which it is the object of the present remarks to summarise were written before the changes lately announced in the terms of service had been made public; it is considered nevertheless that the views expressed by their writers may with advantage be given in this place, as they indicate the nature of measures recommended by professional men. Some time ago the late Director-General, Sir J. B. Gibson, expressed his opinion that the reason why we do not get better recruits is the fact that strong men are able to obtain in civil life better employment as far as remuneration is concerned, than they are in military. It is said that the soldier looks forward to receiving sufficient pension to provide him in old age with the comforts he had been accustomed to, and that it is therefore policy to render his service one of contentment. His dress should be constructed and his training arranged with regard to physiological and anatomical necessities, and the monotony of his duties should be relieved by abolishing, as much as possible, night duty, guards, and other modes of exhausting the soldier's strength without equivalent returns. He should be permitted occasionally to visit his friends for longer periods than he has heretofore been; his wife should have a separate room in barracks; hopes and privileges should be held out to men who desire to work and raise themselves; the privilege of marriage should be permitted under certain conditions, to all, after a fixed time. Under short enlistment, the privilege of permission to marry should be held out on re-engagement; statistics indicating that the rates of sickness and mortality are less among the married men than the unmarried. Soldiers should be educated, and so raised to the condition of other armies, as for example those of the United States and Germany; the government of these countries look for defence, according to Lord Russell, to the education their youth

receive. It is to be observed, however, that by *education* is meant not so much *schooling* as the *formation of character*. The most approved principles for constructing barracks and for selecting sites should be adopted. The men should be accommodated in numbers not exceeding six or eight in each room, having at least 1400 cubic feet of air a piece; to introduce this measure generally, would doubtless be productive of a very large money expenditure. Baths in barracks should be properly fitted up with supply of warm and cold water, and the soldiers made to bathe, if not every day, at least once a week, as prisoners are. In times of epidemics, more especially on foreign service, an increase in nitrogenous articles of food should be permitted, and drills diminished.

Such are some of the measures which seem most needed to effect a practical improvement in the condition of our soldiers, and thereby, in far more ways than I have pointed out, lessen the evil influence of the army on public health.

II.—Insanity in the Lower Animals.—By W. LAUDER LINDSAY, M.D., F.R.S.E., Physician to the Murray Royal Institution (for the Insane), Perth.

“With caution judge of *possibility*.
Things thought unlikely, e'en impossible,
Experience often shows us to be *true*.”

Shakespeare.

IN 1853-4 I was engaged in a series of *experimental studies in comparative pathology*, which led me to entertain a very high opinion of the importance to the physician of a knowledge of the *pathology of the lower animals*, both in itself, and more particularly in relation to a better acquaintance with the diseases of man. The views which I was then led to adopt and express were received by the medical profession, even by some of its most eminent members, very much as the mere vagaries of youthful novelty and enthusiasm.¹ But in the interval—thanks especially to the appearance in this country of the cattle plague of 1865-6²—the rinderpest itself, the diseases of the lower animals as a group, and the important science of comparative pathology, received, from the medical profession, a degree of attention, which has now placed the *pathology of the lower animals* much more nearly on a par with that of man, as regards both our estimate of its importance, and the thoroughness with which it is studied. Physicians and veterinarians now vie with each other, as they should do, in the study of animal pathology, and it is to the former that we owe the best of our knowledge regarding rinderpest and many other animal diseases.³ Nowadays, therefore, the laugh is no longer against the individual observer, who proclaims as a startling novelty the intimate inter-connection of animals and man, as concerns both their pathology and physiology, but

¹ To compare humble persons or small things with greater ones, I might have applied to myself the recent words of Disraeli in the general preface to the new edition of his works (1870):—“He who steps out of the crowd is listened to with suspicion or with heedlessness. . . . I incurred the accustomed penalty in being looked on as *visionary*, and what I knew to be *facts* were treated as *paradoxes*,” or the still more recent utterance of my friend, Professor Blackie, at his last inaugural address in the University of Edinburgh (November, 1870):—“I knew too well that, in small as in great matters, the powers that rule the world are *authority and custom*; sometimes, indeed, beneficially supporting, but at other times perniciously defying, Reason.”

² It lasted about a year, extending over the latter half-year of 1865 and the first half of 1866.

³ For instance, Dr. Smart's *Memoirs on Cattle Plague*; Dr. Claude Bernard's and Dr. Brown-Séquard's *Researches in Comparative Pathology*; and Dr. Greenhow's *Report on Pulmonary Murrain*.

against the imperfectly educated physician or veterinarian who is not fully acquainted with their important inter-relations! In other words, I have lived long enough to see, in regard to comparative pathology, views that were once singular, and the subjects of much ridicule, become thoroughly established, both in science and popular belief.¹

It is again my fate to broach opinions for which, I fear, my professional brethren may not be prepared, and which veterinarians especially will probably regard as equally heterodox and novel. But I have little doubt that a dispassionate study of the subject will, in the course of time, lead to as general a reception and adoption of my present views as occurred in the case of those above referred to.²

For the last three years (1869-71)³ I have been engaged in the study of another department of comparative pathology, one which opens up, it would appear, a wide and novel field of the most interesting and promising kind for original research, viz. that department which relates to the *disorders of mind in the lower animals*.⁴ Those who have given no attention to the subject of reason or intelligence, as distinguished from *instinct*, in animals, will probably require to be convinced that a *mind* of any kind exists in animals before they are prepared to listen to discussion as to whether and how far *disorders* of mind can be properly spoken of as possible in other beings than man. My correspondence and reading convince me that there are very few physicians, and still fewer veterinarians, who are ready to admit or believe that animals possess *mind* at all, referring, as they do, all the operations or phenomena in animals, that would be termed *mental* if they occurred in man, to the very convenient and heterogeneous category of *instinct*. Their non-

¹ Thus, Dr. Rumsey, the well-known advocate of "state medicine," expresses his belief "that a scientific study of comparative pathology is inseparable from medicine in its wider and truer sense; that the principal discoveries in animal disease are due to medical philosophers; and that in practice the subject is essential to a proper organization of state medicine."—*Brit. and Foreign Med.-Chirurg. Review*, July, 1870, p. 231.

² Relating more especially to the transmissibility of *cholera* from man to other animals. (*Vide* the following papers: 1. "On the Transmission of Disease between Man and the Lower Animals," *Edin. Veterinary Review and Annals of Comparative Pathology*, July, 1858; 2. "On Choleraization," *Lancet*, December 1, 1866, p. 600).

³ The subject was alluded to in my paper on "The Causes of Insanity in Arctic Countries," *Brit. and Foreign Medico-Chirurgical Review*, January, 1870, pp. 212, 216, 217.

⁴ Professor Huxley wrote me (in June, 1870), "The line of inquiry which you are pursuing, respecting the *pathological relations of man with other animals*, is very interesting, and I hope you will follow it up." Dr. John Brown, the well-known author of the '*Horæ Subsecivæ*,' calls it "a curious but baffling inquiry" (letter of December, 1869).

belief is as frequently attributable to most irrational prejudice as to mere ignorance. I cannot, in the present paper, undertake to convince the ignorant or prejudiced that the lower animals *do* possess *mind*. A long course of patient and laborious inquiry leaves my own mind in no doubt on this subject, but the results of that inquiry I must reserve for exposition and discussion in some other form.¹ Meanwhile it must suffice to give here only the general conclusions bearing more immediately on the subject of the present paper, which is a consideration of those *mental* or cerebral functional disorders in the lower animals that are comparable with, or analogous to, the group of affections embraced in the category of *insanity in man*.

The kind and amount of evidence I have collected sufficiently convince myself that—

1. Certain of the lower animals possess *mind of the same nature as that of man*.

2. There is therefore *no essential mental*² *distinction* between man and other animals.

3. Many of the same influences that are the *causes* of insanity in man operate, frequently in the same way and to the same degree, on the mind of animals.

4. Man and other animals are alike subject to *other diseases*,³ including especially those of the brain and general nervous system.

5. The same sudden and marked *changes of character or disposition*, that in man so usually constitute the *prodromata* of insanity, occur equally in animals.

6. In animals, as in man, there is *hereditary transmission* of predispositions to disease, of qualities acquired by education, of deformities accidentally produced, of morbid lesions artificially created.

7. The diseases common to man and other animals are frequently at least due to *similar causes*.⁴

8. The lower animals are liable to the *same kind of mental disorders* as man.

¹ I have already published the first instalment of these results in a paper treating of "The Physiology of Mind in the Lower Animals" in the 'Journal of Mental Science,' April, 1871.

² I use the term *mental* rather than *psychological* or *psychical* as being more restricted in the meaning usually attached to it. *Mind*, as defined by Professor Bain and our best modern authors, includes only Emotion, will, and intellect; and it is in this sense that throughout the present paper I use the terms *mind* and *mental*. But this definition of mind *excludes instinct* and *sensation*, both of which are so intimately connected with mind that it is impossible to dissociate them. It also *excludes self-consciousness, soul* and *spirit*, which are usually regarded (with what degree of truth I cannot here stop to inquire) as the special attributes of humanity.

³ *Vide* Gamgee in 'Edinburgh Veterinary Review and Annals of Comparative Pathology,' vol. iii (1861), p. 3.

⁴ *Ibid.*, p. 4.

9. In comparing the mental or other diseases of animals with those of man, due allowance must be made for *ordinal*, *generic*, and *specific*—for anatomical, physiological, and therefore, also, pathological—*differences*; as well as for *individual idiosyncrasies* or predispositions.

I do not profess in the present paper to be able to adduce

“*Proofs*—as clear as founts in July,
When we see each grain of gravel”

that the lower animals are subject to *all* the forms of insanity that afflict man. But I hope to be able to submit evidence of the likelihood that sufficient *proof* will be forthcoming, when it is properly searched for—that man and certain other animals are subject *in common* to various forms of *mental disorder*. My own position does not give me the opportunity of collecting records, or investigating cases, of animal cerebral disorders that are likely to prove analogous to or comparable with what is called *insanity* in man. My paper claims, therefore, only to be *suggestive*. It is intended only to stimulate to research those who have the greatest natural facilities for, or opportunities of, collecting valuable evidence, viz. *veterinarians*. When I began my inquiry I looked to *them* for assistance in its prosecution; but I soon found them, not only more ignorant, but also more prejudiced, than I could have conceived possible, considering their intimate knowledge of the habits and disorders of at least the higher domesticated animals. One eminent veterinary professor informs me that animals have *no* mind, while several others announce their inability to furnish me with references to any cases of functional cerebral disorder in animals at all comparable with insanity in man.¹ All veterinarians, however, admit that they have given no attention whatever to the mental affections of animals.

I have met but with one (apparent) exception to the ignorance or prejudice of veterinarians on this subject. Professor Gamgee admits that “*much* could be said on the subject of *morbid mental* conditions in animals,” but that, in short, “the history of nervous diseases in the lower animals has yet to be written;” and he goes on to explain, in relation to morbid mental conditions, “the ferocity suddenly manifested by animals which early in life have been very docile, the extreme irritability of some, and the apparently gross stupidity and listlessness of others, often indicate deviations from the normal state of either *instinctive* or *reasoning*

¹ Thus, Professor Simmonds, of the Royal Veterinary College, London, wrote me in January, 1870, “I do not know of any cases to which I can refer you.” Similar was the result of an application to Mr. Charles Conacher, veterinary surgeon, Perth; while Dr. John Brown, who was intimate with the late Professor Dick, and who is noted for his knowledge of the habits of the dog, wrote me, in December, 1869, “I fear I can help you little or none in this curious but baffling inquiry; I don’t know any references.”

faculties. There are singular instances noticed of animals acquiring peculiar morbid tastes, which can only be explained as due to nervous disease; and there can be no doubt that, just as we find the greatest diversity in the amount of intelligence possessed by different individuals of the same species, so may we have perversions of *instinct* or *mind* similar to those which are manifested by the human idiot or lunatic."¹ The connection between diversity in the amount of intelligence and perversions of instinct or reason is here far from apparent, and much of his language, meaning, or assertion is confused, obscure, or erroneous; but it is obvious that it had occurred to him to compare certain *morbid mental* phenomena in the domestic animals with some of the forms of human *insanity*. It so happens that Professor Gamgee's work was one of the last I had an opportunity of consulting, and that I had worked out my own conclusions in opposition to the general opinions of veterinarians before I had access to these views of his. Such as they are, I now hold them as *pro tanto* supporting from a veterinarian's point of view the conclusions at which I had previously arrived as an alienist and a physician.

On the other hand, almost the sole support to my convictions I have received from the medical profession has been the following testimony of an author, whose affection for and intimate knowledge of the dog has been amply evidenced in 'Rab and his Friends.'² I refer, of course, to Dr. John Brown, of Edinburgh. In December, 1869, he wrote me, "Surely if animals have will, and thought, and affection, they may, in their modes, or functions, or organs, be *disordered* and *deranged*, as truly as are poor humans!" In his chapter on "Our Dogs," in his 'Floræ Subsecivæ,'³ he speaks of a certain "Jock" as "*insane* from his birth . . . more properly *daft* than mad;" and of a colt, which, "being nervous, *lost its judgment* . . . and presence of mind."⁴

The fundamental question, which obviously underlies and forms the basis of my whole inquiry, is this, *Have animals a mind similar to that of man—capable of being disordered by the same influences?* In endeavouring to solve this problem I have carefully studied all the most recent and best works to which I have had access on the habits, character, and disposition

¹ 'Our Domestic Animals in Health and Disease,' vol. iv (1864), p. 450.

² Dr. Tuke apparently recognises the occurrence of *insanity* in animals, inasmuch as he speaks ('Manual,' p. 507) of insanity "remaining at a minimum among barbarous nations, as well as among children, and *animals below man*." This estimate of the infrequency of insanity in the lower animals is, however, I believe, erroneous.

³ Second series, 1861, p. 159.

⁴ *Ibid.*, p. 157.

of animals; noting all those peculiarities, good or bad, healthy or morbid, that in man would be referred to or connected with *mind*. On the other hand, I have equally carefully studied the definitions of Mind given in our best modern standard works; and, comparing the mental faculties of man with the phenomena of so-called intelligence or sagacity, educability, affection, emotion or passion, &c., in animals, have endeavoured to discover whether in these animals are absent any of the faculties which go to make up the human mind. Whatever may have been the definition of mind, or the classification of the mental faculties, of Man examined, I have never failed to satisfy myself that the lower animals, or certain of them, possess in varying degree *all* the faculties of the human mind. One of the most concise and modern descriptions of mind in man is that of 'Chambers's Encyclopædia,'¹ the articles in which are contributed by some of the highest living authorities. According to it, the three elementary constituent faculties or functions of the human mind are—

- (1) *Emotion, or feeling*;
- (2) *Volition, or will*; and
- (3) *Intellect, or thought*.

Intellect, again—the thinking portion of the mental constitution—involves or includes *reasoning, comparison, memory, imagination, perception, reflection, and judgment*. There are *none* of these mental powers or properties, which are not possessed by some of the lower animals, especially by the dog, horse, elephant, whale, ant, bee and spider; while some of them are possessed *in a very high degree*—in a degree much higher, indeed, than by countless thousands of human beings!

I have found, however, the *phrenological* classification of the mental faculties a much more convenient standard than any of the more modern classifications of psychologists, the more especially seeing that the phrenologists distinguished themselves by their advanced, enlightened and liberal opinions in recognising *mental power in animals*, and in comparing this power, at least in health, with the human mind. In George Combe's 'System of Phrenology,'² there is a section on *comparative phrenology*,³ which is equivalent, so far, to the modern *comparative psychology*, but which scarcely includes *comparative psycho-pathology*. According to phrenologists, animals possess *all the mental faculties of man*, whether (1) *affective*, or (2) *intellectual*. Thus, they possess the following:

¹ Articles "Intellect," "Mind," "Instinct."

² Fifth edition, 1853.

³ Ibid., vol. ii, p. 382.

I. *Perceptive faculties.*

Individuality, weight (resistance), locality, order, eventuality, form, size, time, tune (musical talent), number, and language.

II. *Reflective faculties.*

Comparison, causality.

III. *Propensities.*

Secretiveness, destructiveness, philoprogenitiveness, alimentiveness, combativeness, inhabitiveness, concentrativeness, adhesiveness, amativeness, acquisitiveness, and constructiveness.

IV. *Lower sentiments* (common to man and the lower animals).

Self-esteem, love of approbation, and cautiousness.

While believing, with the phrenologists, that certain of the lower animals exhibit, in some degree, *all* of these mental phenomena or attributes, there are *some* of the so-called faculties, whose existence in animals is capable of more ready and abundant evidence than others, while the occurrence of a few is so obvious as to require no comment.

The phrenologists recognise, however, a group of—

V. *Superior sentiments peculiar to man*—the so-called *moral sentiments*, which include—

Benevolence, veneration, firmness (perseverance), conscientiousness, hope, wonder, ideality, wit (mirthfulness), and imitation.

It is, I believe, a decided error to suppose that these faculties are not possessed by animals. There is quite as much, and as good, evidence in support of the belief that they exhibit these *higher* sentiments as that they possess the *lower* ones, or the propensities that are generally, but most erroneously, denominated, if distinctiveness is pointed at, *animal*.¹ There is good ground for believing that certain of the domestic animals possess what is known as "*the moral sense*" or "*the feeling of moral obligation*" in man. They can be at least trained to a knowledge of *right and wrong*,² and to an intelligent com-

¹ Dr. Take writes, "Animals possess, in degree, so many of the moral faculties, which man prides himself on possessing, and man is so largely influenced by the propensities, . . . that the term '*animal*' cannot be employed in a very strict sense" ('Manual,' p. 87).

² "Animals have a sense of *right and wrong*," says Clayton, p. 220. Ants have *codes of laws, and punishments* for their infringement. "In their natural state," says Clayton (p. 230), "animals erect *moral laws*, and institute *punishments* on their infringement."

prehension of *praise and blame* for well or evil doing, and of the principle of *rewards and punishments* therefor. There is abundant proof of their also possessing *benevolence* and generosity, solicitude for, compassion or sympathy with, human joys and sorrows, as well as with those of their own kin or species; and there can be no doubt as to their powers of *imitation*. There are therefore *moral*, and there are also *immoral*, qualities in animals; there are both good and bad features in their "characters" or "dispositions," using all these terms in the popular sense in which they are generally applied to man. Their bad qualities include, for instance, anger, hatred or antipathy, quarrelsomeness, bad temper, retaliation, revenge, vindictiveness or the resentment of injury, unforgiveness, petulance, impatience, mischievousness, theft, cowardice, ferocity, viciousness, moroseness, intractableness, selfishness, obstinacy or stubbornness, spitefulness, sullenness or surliness, cruelty, tyranny, laziness. It will suffice, further, to illustrate at once the comprehensiveness of the animal mind and its similarity to that of man, to mention that certain of the domestic animals are capable of exhibiting a wonderful degree of self-command; *amour propre*; a keen sensibility to ridicule, affront, insult; a sense of shame, discomfiture, defeat, disgrace, detection in wrong-doing; agony, distress, remorse, misery, unhappiness, annoyance, uneasiness; emulation, sometimes amounting to fierce contests for supremacy; eagerness, impetuosity, ardour, vehemence, personal rivalry, apprehension, fear; pleasures and pains, joy and sorrow, excitement and depression.

The chief *causes of insanity* in man, as laid down in one of our latest text-books on the subject (Bucknill and Tuke's 'Manual of Psychological Medicine,' 1858, p. 241), are as follows:

I. *Predisposing.*

Hereditary, sex, age, season, town and country life, occupation.

II. *Exciting.*

A. *Moral*: disappointed affections, domestic troubles, wounded feelings, fright or fear, grief, anxiety, or jealousy.

B. *Physical*: general ill health, specific injuries or diseases (*e.g.* epilepsy and uterine derangement), pregnancy.¹

¹ Compare also the section on *causes* in Prof. Aitken's 'Science and Practice of Medicine,' 4th ed. (1866), vol. ii, p. 408; and in the late Sir William Charles Hood's 'Statistics of Insanity,' 1855, p. 52, and 1862, p. 53.

There are very few of these causes or influences that do not, in some measure, operate also in the case of animals, while some of them operate in much larger measure. Certain causes may be held peculiar to man, for instance religious excitement and intemperance; but, on the other hand, there may be some that are peculiar to other animals. I have elsewhere already shown that certain of the same influences that injuriously affect the mind of man in arctic countries equally affect that of the dog.¹

The important subject of *hereditary transmissibility* of healthy and morbid peculiarities has probably not been studied to the same extent among the lower animals as in man. But it has been sufficiently studied to show that no less interesting and valuable results are to be looked for from a full knowledge of the subject among the lower animals than in man. Quite recently some of our most eminent experimental physiologists and pathologists have been directing their attention to this fertile field of inquiry, and already it has been proved that various singular deformities in structure, and various mental peculiarities—the fruits of education—are capable of hereditary transmission, as well as are predispositions to disease. At the last meeting of the British Association for the Advancement of Science (1870), Dr. Brown-Séquard showed that *epilepsy* can easily be *produced artificially* in animals (*e.g.* the guinea-pig); that the disease so created is capable of hereditary transmission; and that other diseases of the nervous system or morbid conditions depending on depressed nervous action,² in animals, are equally capable of hereditary transmission.

In the article “Horse,” in ‘Chambers’s Encyclopædia,’ it is stated that domesticated peculiarities—*mental susceptibility* as well as corporeal symmetry—steadiness of habit, and other qualities valuable to man, are transmitted hereditarily. The writer of the “Anecdotes of Dogs” in ‘Chambers’s Miscellany’³ remarks it as curious that the *results of instruction* are in them hereditary. There is, indeed, no reason to doubt that, just as in man, *talent* is—at least frequently—hereditary, it is equally so in the lower animals, *e.g.* the talent for mimicry, song, speech, &c., which are the results of high cultivation of their natural aptitudes.

The effect of *domestication* and *breeding* in the production of

¹ Especially the influence of the long and intense darkness of the arctic winter. (*Vide* paper on “The Causes of Insanity in Arctic Countries,” ‘Brit. and Foreign Medico-Chirurgical Review,’ January, 1870, pp. 212 and 216).

² *E.g.*, dry gangrene of the ear in epileptic guinea-pigs—a condition somewhat parallel to the *Othæmatoma* of the insane, formerly supposed due to ill-treatment by asylum-attendants.

³ Revised edition of 1869, vol. i, p. 24.

a *predisposition* to disorders of the whole nervous system—including especially the brain—is another subject of the first importance in relation to the *etiology of insanity in animals*. High breeding, for instance, in dogs and horses, begets extreme nervous sensibility, including *high mental sensitiveness*; one result whereof is great susceptibility to disorders of the whole nervous system. The result produced is, in fact, analogous to that which is the effect of *high civilisation in man*.¹ The distinguished Parisian experimental physiologist and pathologist, Claude Bernard, has pointed out the effect of breeding in the obvious differences, as regards nervous sensibility or irritability (using these terms in their strictly scientific signification), between artificially (high) bred animals and the same species, or even the same individuals, in the wild or natural state.² He remarks, “We may, I believe, take it for granted that not only *morbid*, but also physiological, *predispositions* exist in man as well as in the lower animals.” (p. 109) “These important modifications are almost inevitably produced through the agency of the *nervous system*.” (p. 110) “So exquisite is the nervous sensibility of dogs of the higher breed, that the slightest operations bring on fever and are attended with alarming symptoms. They cannot therefore be employed in researches connected with the gastric juice: in fact, all operations performed within the abdominal cavity are liable to superinduce peritonitis in these highly sensitive animals, and generally prove fatal. In dogs of a more vulgar class how different are the results of similar experiments?” (p. 110) “In the horse these differences are, if possible, still more strongly marked It is therefore indispensable, whenever great powers of endurance are required for the purposes of scientific research, to select an animal of the lower breed; and if, on the contrary, sensitiveness and nervous irritability appear desirable, none but the nobler kinds will afford the requisite qualities. Experiments on recurrent sensibility, for instance, which in the greyhound and pointer are generally successful, if tried on a shepherd’s dog would fail in almost every case.” (p. 110.) Professor Gamgee, too, tells us that “*great nervous excitement*” attends various

¹ Dr. Tuke (‘Manual,’ p. 36) writes, “There is an acuteness of sensibility, a susceptibility of the emotions, an intense activity of the feelings, which would seem to be peculiar to *highly civilised life*. . . . It would seem obviously to follow from this cultivated condition of the higher sentiments, that external circumstances are much more likely to produce an impression upon them, to excite or depress them, and thus induce a shock under which the *mind* at last succumbs.”

² “Lectures on Experimental Pathology,” chapter on “Idiosyncrasies in Animals,” ‘Medical Times and Gazette,’ vol. i, for 1860, p. 109. *Vide* also 36th Report of the Murray Royal Institution, p. 69.

disorders of animals, *e.g.*, the horse; while Jesse, in his 'Anecdotes of Dogs' (1867), relates that in *domestication* the dog has "wants and wishes, hopes and fears, joys and sorrows, to which in his wilder state he appears to have been a stranger." (p. 42.)

Professor Aitken writes, "There is no description of insanity, which, if traced to its source, may not be found either to consist in *perverted emotion*, or to emanate from that origin." (p. 409.) Now, many of the lower animals, especially those which are domesticated, are *eminently emotional*.¹ Jesse specially mentions this in regard to the dog. It is capable of exhibiting extravagance of behaviour both in joys and sorrows. He describes its "screams of gratitude and joy" (p. 307), and signs of "most extravagant joy." Much more frequently, however, its *grief* is extreme, resulting from the sundering of its attachments to man, or other animals. Not a few instances have been placed on record of *death from grief in the dog*; while cases of *suicide* from the same cause will be found cited under the head of *melancholia*.² One of the most recent instances I have met with of *death from grief in the dog* is the following "Pathetic story of two dogs," as related in the San Francisco newspapers. In that city, it would appear, there lived for some time two dogs, "Bummer" and "Lazarus," which acquired quite a popular celebrity on account of their remarkable friendship for each other. After having long been inseparable companions, Lazarus died; while Bummer, "after a few days of foodless, moody grief, died also, and was buried beside the friend whose loss had been more than his dog's heart could bear." Bummer had originally played to Lazarus the part of the "Good Samaritan." The history of their friendship became known throughout the city; and the characters of the two dogs were so much esteemed that a public monument was, after their death, erected to the "memory of two friends who were lovely in their lives, and in their death are not divided!" The case of Bummer may legitimately perhaps be considered a *suicide by starvation*. Jesse relates instances of death from grief at the loss of master, mistress, or companion, in the *dog* (pp. 318 and 320) or at being left behind on a journey or change of residence (p. 457), in which he speaks of symptoms of "*broken heart*" (p. 320). He also describes the dog as expressing its grief by "*mournful cries*," or by mournful

¹ Bates, 'The Naturalist on the River Amazon' (1863, p. 212) mentions a Coaita monkey "which evinced the *most painful emotions* at being called hard names by its owner—seating itself quietly on the ground and *crying piteously*." What more could a sensitive child do?

² I have associated *suicide* with *melancholia* rather than with *emotional insanity*, because this act is usually preceded in animals by melancholia, of which, indeed, it may be considered one of the most frequent symptoms or concomitants—one of the most common and natural terminations.

looks and attitude at the death of a master, mistress, or play-fellow (p. 450); and the bitch as frequently "*disconsolate*" for the loss of her whelps. Various other animals are known to contract friendships with man, or with animals of the same or other species, of such a character that their sudden cessation has produced a fatal degree of grief. Menault¹ (p. 281) cites cases of *death from self-starvation* among horses from loss of their companions, other horses; and he gives also an instance of a parrot. Jesse mentions the death of a partridge from grief at loss of a companion, a dog (p. 314); and cites the case of a cock becoming "*melancholy*," also from loss of a companion, a dog (p. 294). Menault states that the goldfinch has died of grief at loss of a companion prisoner, a canary. In Ceylon, elephants sometimes die of what the natives call a "broken heart."

The passion of *love* is quite as absorbing in many animals as in man, and it is equally liable to disturbance and disorder. Its intensity is but partially expressed in the song of certain birds at the pairing season. The maternal instinct, the natural domestic affections, general brotherly love, and even the high sentiment of patriotism, are frequently quite as powerful in other animals as in man; and any sudden or serious interference with the legitimate gratification of these affections is liable to produce in them the same *kinds* of results. Menault mentions that the social affections of the whale, when interfered with, lead to the development of determined courage and of the fury of desperation. As characteristics of the swallow, he describes conjugal fidelity, maternal devotion, social love, filial gratitude, generous provision for orphans, sympathy for the welfare of the race [or patriotism], and affection for birth-place or scenes of first love. *Jealousy* also sometimes acquires the position or power of a master-passion in some of the domestic animals. Thus, Jesse says, "Some dogs are of an extremely jealous disposition," and he gives instances of their jealousy prompting them to the *murder* of other animals (pp. 170, 171). Cases are known of one dog worrying another that had, in its estimation, monopolised, for the moment, its mistress's attention or caresses. Referring to such a case, some versifier writes—

"Oh, reader! here you'll truly find
The portraiture of *human kind*:
The proud, the destitute of sense,
Blinded by power, *will* take offence
Where none is given; and use their might,
Nor question whether wrong or right!"

In the struggle for existence, many animals, especially those

¹ 'The Intelligence of Animals,' translated from the French, 1869.

against whose life man wages perpetual war, are subject to the extremest strains of an *anxiety*, which includes the emotions of apprehension or fear, terror or alarm. Leroy¹ describes the wild life of the carnivora as "full of interest and of fears, which in some measure represent the agitations of civilised man."² The various classes of snares set for them by man keep them in a condition of incessant fear.

Maltreatment by man, as well as the cruelty or tyranny of other animals, are fertile causes of deterioration in the character or disposition of animals. There is too much reason to believe that many, at least, of the *bad* qualities of the animal mind or character are due wholly to *association with man*, and to the influence of a usage, which it would be a libel upon the lower animals to term *brutal*. Thus, the writer of the article "*Horse*," in 'Chambers's Encyclopædia,' remarks of the proverbial obstinacy of the *ass*, "In most instances this stubbornness is the result of *bad treatment*—a fact that says less for the *humanity* and intelligence of man than for the natural disposition of the brute" (p. 15). In the wild or natural state, Menault tells us, this animal displays energy, activity, courage, and discipline. The *mule* also exhibits, under favorable circumstances, traits of a very opposite character to those usually assigned to it; as does also the *pig*, which is represented by some authors as "naturally a very clean animal," its love of dirt, of "wallowing in the mire," being attributed to bad usage by man.³ In regard to the *dog*, Jesse tells us that "ill-usage makes him sullen and distrustful of beings far more *brutal* than himself" (p. 16)! His faults are easily checked, but if used roughly he is apt to become sulky (p. 177). On the other hand, "*society and culture* soften and moderate their passions" (p. 25); a strong argument in favour of proper training or education, as well as good usage, for there can be no doubt that the "*character*" of domesticated animals may be said to be, in great measure at least, very much that of, or formed by, their masters. Animals are often tyrannical or cruel to each other; and it is known that some of them *punish* their young, or each other, for such faults as idleness,⁴ stupidity, theft, and desertion of trust.

¹ Charles Georges Leroy, a Frenchman, who was ranger of Versailles and Marly (near Paris) about the middle of last century. He wrote, partly under the pseudonym of 'The Naturalist of Nuremberg,' a series of letters, which have been recently translated and published in London (1870), under the title of 'The Intelligence and Perfectibility of Animals, from a Philosophic point of view.'

² Menault, p. 297.

³ Ibid., p. 294.

⁴ Jesse, p. 420. Elephants in India, for instance, *punish* each other, when so directed by their *mabots* (drivers), "by thrashing with a heavy chain, wielded with tremendous effect by their long trunks." (Chapter "On the possible future

Maltreatment by man varies, both in kind and degree, from that which is habitual and long continued, to that which is a mere temporary provocation or irritation; from that which springs simply from ignorance or thoughtlessness to that which is cruelty, wanton and deliberate. The latter category includes mostly the cruelty inflicted on animals in connection with the so-called "sports" of man—bull-baiting, badger-baiting, cock-fighting, dog-fighting, and the like. In the dog, Jesse states that continued provocation or irritation causes loss of its usual patience or forbearance, its self-command or control over its passions, and develops a "*ferocious temper*," leading it sometimes to the *murder* of other dogs. In other words, it may be considered that a passion equivalent to homicidal mania is generated. We read of other animals being "irritated" or "*goaded to fury*;" and it would appear that they may be "*tortured into madness*," just as, in the days of old Bedlam, human beings are said to have been so excited by tickling the soles of the bare feet. To the category of maltreatment by man also belongs, in part at least, the *captivity* of wild animals, which, especially in the case of certain of the carnivora, cannot fail to be a source of the most intense irritation, opposed, as it is, to all their natural habits and suitabilities.

Solitude—especially in animals accustomed to the society, whether of their kind or of man—frequently produces change of disposition to moroseness or viciousness,¹ *e.g.* in the "rogue" elephant, buffalo, hippopotamus, and sperm whale; that is, in animals, which for some reason are expelled or outlawed from the herds to which they naturally belong. *Cold*,² intense or prolonged, would also appear to affect the animal, as it does occasionally the human, mind. Thus, Jesse mentions the case of a setter (dog) *going mad*, "possibly in consequence of his exposure to the severe frost of the night" (p. 404.) Nor is there reason to doubt that *hunger* and *thirst*, or the use of improper food and drink, uncleanness of person or habitation, insufficient ventilation of dwellings, monotony of occupation, and many other indirect causes of mental deterioration or disorder in man, may and do, *mutatis mutandis*, also affect the mental constitution of the lower animals. Though a most un-

Existence of the so-called Brute Creation," in Capt. Clayton's 'Scenes and Studies,' 1870, p. 208.)

¹ This term, as used by writers on the dispositions of animals, probably means mere irritability of temper or irascibility, which, however, may pass into *mania*.

² I have given illustrations of the influence of solitude, cold, darkness, and starvation, which are apt equally to affect the mind of man and other animals, in my paper on "The Causes of Insanity in Arctic Countries" (formerly quoted).

likely *cause of insanity* in animals, it is right, nevertheless, here to mention the fact that *alcohol* produces in some of them the same effects that it does on man, and that they have occasionally been seen in a state of genuine alcoholic intoxication. Thus, Menault describes fondness for wine as a characteristic of the parrot, which under its influence becomes tipsy, dancing and attitudinising as clumsily as an inebriate man would. The *poisonous stings of insects*, in foreign countries, sometimes *torture* horses and other animals (*e.g.* in Africa) into *madness*,¹ but, as will be afterwards shown, this so-called "madness" is not necessarily of the kind to which the designation "insanity" is usually attached in man.

No consideration of the *Etiology of insanity in animals* is complete, which does not regard *individual idiosyncrasy*. To this category apparently belongs the excitement produced in certain animals by particular *sounds* and *colours*. Jesse tells us that, in the dog, *music* sometimes produces intolerable pain or disquiet, or agony is gendered only by false notes. Some so-called highly-educated "musical" dogs have been stimulated or excited to utter loss of self-control, even to the most *dangerous fury*, by false notes accidentally or intentionally produced, *e.g.* in the case of the well-known "musical" poodle of Darmstadt. (p. 80.) The proverbial irritation to fury producible by the exhibition of a bright *red colour* to the bull, is a parallel illustration, if the excitement so producible is founded on fact and not apocryphal!

It would appear, then, that, while the lower animals are subject to most of the influences that produce insanity in man, the more frequent or prominent *causes of insanity* in them are—

I. *Predisposition*—produced by
 Previous attacks,
 Hereditary transmission,
 Domestication,
 Breeding,
 Idiosyncrasy.

II. *Maltreatment*, by man.
 Captivity,
 Direct ill-usage.

III. *Excessive emotion*.

IV. *Undue excitement of the passions*.

¹ I have given cases of *madness* in goats, sheep, and cattle, leading sometimes to *suicide* by drowning or otherwise, produced by the narcotico-irritant action of a poisonous plant, in my paper "On the Toot-Plant and Poison of New Zealand," Brit. and Foreign Medico-Chirurg. Review, July, 1865, pp. 154, 166, 168.

V. *Physical* influences, connected with climate or season.

VI. *Diseases* of the general system; or specific disorders of the cerebral, nervous, digestive or uterine systems.

In judging of the probable effects of the operation of such influences on the mental constitution of animals, we must not expect the *same causes to produce the same results* as in man. Due allowance must be made not only for the remarkable *differences between man and other animals* in habits of life, in their anatomy, physiology and pathology; not only for the peculiarities, structural and functional, of races and breeds, of orders, genera, and species; but also for the idiosyncrasies of individuals. "We must not expect," says Menault (p. 113), "in creatures, whose cerebral organs differ from ours, intellectual manifestations equal to our own," either in health or disease. Animals necessarily use or exhibit their faculties in a different way and for different ends. The most signal difference between man and other animals consists in the possession by the former, and the absence in the latter, of articulate *speech*,¹ and of a *hand*, having fingers endowed with a fine tactile sensibility, and a thumb opposable to the fingers. Newfoundland dogs, Jesse asserts, "only want the faculty of *speech* to make themselves fully understood." (p. 168.) The author of the article *Elephant* in 'Chambers's Encyclopædia' remarks that it is only because man is gifted with *speech and a hand* that he excels animals in some respects as much as he is inferior in others. It is obvious that the *want of speech* must prove a serious obstacle to the detection of delusion, or of insanity at all, in the lower animals. As regards the *hand*, Professor Goodsir writes, "the peculiar manner in which the human thumb can be opposed to the fingers, and the entire hand folded around the object, as well as the *specifically human* manner in which the upper limbs can embrace an object, or enclose a space, are no doubt related to the requirements of the human self-consciousness." (p. 321.)² Menault points out that where animals have hands or feet at all like those of man, they can be taught to apply them to many of the same purposes, such as eating at table, sitting, standing or walking (*e.g.* the chimpanzee and ourang-outang). The elephant could not be taught to perform many of his feats

¹ Or, as the poet has it,

"Since man from beasts by *words* is known,
Words are man's province."

Of *animal reason* Clayton remarks that it requires "only *speech* to elevate it perhaps to a level with our own!"

² Note on "Psychological Science" appended to the chapter on "Life and Organisation," in his 'Anatomical Memoirs,' 1868, vol. ii.

“were it not that he possesses the wonderful grasping powers of the trunk, which, in this respect, is all but equal to the human hand.” He surpasses other animals in certain exhibitions of skill “only because he is furnished with an instrument of higher capability.” (p. 17.)¹ We know that the genus *dog* alone includes *many distinct races differing in instincts or dispositions*; while Claude Bernard² tells us, “*each particular species of animal has its own peculiar diseases.*” He points out also that “the vitality of the skin in animals is essentially different from that of man.” (p. 50.) . . . “Even in a perfect state of health, *each individual* retains his own peculiar habit of body, and is in consequence more liable to certain accidents than his neighbour. The various animals, which serve for our experiments, are far from exhibiting the same phenomena under the influence of agents entirely similar in their nature” (p. 109). . . . “Not only do the *various species* of animals differ in this respect, but even *individuals belonging to the same species* are so far from resembling each other that they cannot be submitted to the same experiments. . . . It will, therefore, easily be conceived that a state which, in certain animals, would constitute actual *disease*, may be perfectly *natural* in others. . . . *Idiosyncrasies* are only peculiar susceptibilities, which exist in the normal state in various individuals. . . . If we compare an animal in a state of abstinence to one in full digestion, the most evident discrepancies will be noticed in the results of experiment simultaneously performed upon them. A dose of strychnia, which almost immediately kills the second, will not act before the lapse of a certain time upon the first.³ . . . The lowering of the physiological activity of the nervous system is in reality the only cause to which the difference can possibly be referred.” (p. 110.) . . . “But we also find in animals various *predispositions*, which not only modify the action of medicines administered to them; but also render them liable to *diseases entirely different from causes entirely similar*. Being about to perform certain experiments on animals kept fasting for a long space of time, I left some dogs without food for several days.

¹ Art. *Elephant*, ‘Chambers’s Encyclopædia.’

² Than whom no living experimentalist is better entitled to speak with authority on the subject of *idiosyncrasy and predisposition in animals*.

³ I pointed out some remarkable idiosyncrasies of animals, in relation to the *non-action of certain poisons that are fatal to man*, in the following papers:—1. Experiment- with “*Belladonna*, as an Antidote to Opium,” showing the *non-susceptibility of some of the lower animals to the action of certain poisons*, ‘Association Medical Journal,’ June 9, 1854; 2. “*Toot Poison of New Zealand*” (formerly quoted), pp. 161, 169, 170, 171, 175, 176, 177; 3. “On the Properties and Products of the Toot-Plant of New Zealand,” ‘British and Foreign Medico-Chirurg. Review,’ Oct., 1868, pp. 470, 471.

But, during the late severe frosts, these animals died unexpectedly. In making the autopsies, we discovered pneumonia in one case, pleuritis in another, and inflammation of the bowels in the last." . . . "*Morbid predisposition* must therefore be viewed in the light of peculiar physiological conditions, which in most cases depend upon the *nervous* system. And an immense progress would be realised in medicine if it were possible to diagnosticate, in a state of health, the *predisposition to disease*, and foretell the coming danger." . . . *Idiosyncrasy* must then be regarded as the "mere natural manifestation of the ordinary laws of physiology" (p. 111).¹ Professor Dick² also points out that the form, in which cerebral excitement is manifested, is determined by the instincts and habits of different animals. Thus the horse, when unduly excited, bites and *kicks*, the ox *gores*, the ram *butts*, the dog and fox *bite*.

Certain of the lower animals, especially those which are domesticated, are subject to many of the *same diseases of the brain or nervous system* that are so common in man; and hence arises at least the probability that they are equally liable to those functional cerebral disorders that are productive of, or that constitute, *insanity* in man. According to Professors Dick and Gamgee, the following cerebral or nervous disorders occur in the lower animals:—*Epilepsy* occurs in all animals, especially in young dogs. We have already seen, moreover, that it can be artificially produced, and that, when so produced, it is capable of hereditary transmission. There is every reason that we should expect to find associated with epilepsy in other animals the marked changes in temper, or the exhibition of paroxysmal furious mania, that are so common concomitants of the same disease in man. *Catalepsy* is exhibited occasionally in the dog, wolf, and horse. *Chorea* is met with frequently in young dogs, and occasionally in horses and cattle. Various animals are also more or less subject—to *apoplexy*, *coma*, *convulsions*, *hysteria*; *meningitis*; *phrenitis*—the "mad staggers" of the veterinarian; *paralysis* (both spinal and cerebral), and *delirium*; with other diseases of the cerebral, cerebro-spinal, or general nervous systems, obscure in their nature and origin, and not admitting of accurate classification. Moreover, post-mortem examination of animals frequently reveals the existence of *cerebral* or *nervous lesions*, of the same

¹ As regards *man*, Dr. Bucknill remarks, "No one brain is like any other brain. Either by the force of inheritance from parent organism, or through the influence of education, or other modifying circumstances, every mind possesses such a *peculiarity and individuality* in the relative susceptibility and strength of its organs that the same disturbing influence never produces in two brains exactly the same pathological effects" ('Manual,' p. 371).

² "Occasional Papers on Veterinary Subjects," 1869.

character as those which so frequently occur in man. These morbid conditions or lesions include *congestion, induration, softening, atrophy* and *tumours*, of the brain, and *hydrocephalus*. In short, there are probably few functional morbid conditions or organic lesions of the brain, spinal cord, or other parts of the nervous system, that are not as liable to occur in the lower animals as in man.

Further, the development of these morbid conditions and organic lesions is equally frequently accompanied in the lower animals by the exhibition of *mental disturbance*—cerebral functional derangement. Thus *excitement*, with or without delirium, is common in many of the same diseases in which it usually occurs in man, as well as in disorders that are regarded as more specifically peculiar to animals.¹ Sometimes there is *extreme sensitiveness to external impressions*—sound, light, touch, smell; in other words, there is a perverted or excited condition of the *special senses*. Thus the animal may be dangerously excited by any *noise*, even the rustling of straw, or by the mere *touch* of its body. Gamgee speaks of “aberration of the cerebral functions” in phrenitis (p. 482), while he describes atrophy of the brain as also accompanied by “aberration of the *instinctive* faculties, and sometimes by viciousness” (p. 492). It is probable that he here speaks of the “instinctive” faculties merely in deference to erroneous popular opinion, the faculties in question being really what in man would be assigned to mind; and what he calls “viciousness” is, perhaps, really a stage or form of what in man would be called *mania*?

Many of the lower animals exhibit the same sudden, apparently causeless, and marked, *changes in disposition or habits* that constitute the *prodromata* of insanity in man.² These phenomena include the development of perverted or depraved appetite, of alterations in the affections and temper, of remarkable change of the natural or normal habits of the individual. It has been frequently noted that the horse, dog, cattle, and other domestic animals become “bad-tempered” or “curious in their ways”—that they exhibit “viciousness,” or “look wicked” prior to the development of various diseases of the brain or nervous system. Thus Gamgee mentions the display of temper

¹ The number of diseases absolutely confined to the lower animals, and thereby really peculiar to them, is, I suspect, very small. In my paper on the “Transmission of Disease between Man and the Lower Animals” (formerly quoted), I showed how many animal disorders are capable of also affecting man; and our knowledge of the diseases common to man and other animals has made such progress since 1858, that my former catalogue might now be largely added to.

² “One of the most striking features of insanity in general, and the strongest proof of the presence of any of its forms, is the *change* which takes place in the *individual’s character and habits*.” (Bucknill and Tuke’s ‘Manual,’ p. 180.)

and *eccentricity* in an apoplectic horse prior to its seizure, and he describes the "peculiar fierce look" associated with "sleepy staggers" also in the horse. The same author mentions *depraved appetite* as occurring in various forms of cerebral and spinal paralysis. Jesse describes the dog as becoming "cross and snappish" when petted, just as children do; and the same change from a naturally mild, patient disposition occurs in various disorders of that animal. Disappointment is evidenced by sudden and unusual quietude and lack of spirit. A "surly disposition," where not natural, is frequently developed by ill-usage. It is quite common for dogs and other animals, in their excitement, to attack, and even *kill*, the companions of which they had previously been fond.¹ Dick gives an instance of this kind in a dog reputedly *mad* (p. 361). Other changes of character or disposition are to be found in the development of laziness, apathy, or stupidity;² in the loss of natural animation or vivacity; in the exhibition of fits or paroxysms of passion, rage, fury; or of general nervous excitement, or irritability.

Eccentricities, not amounting sometimes to insanity, but quite comparable at least with the eccentricities of man, are not uncommon in the lower animals. Hogg, the "Ettrick Shepherd," speaks of the "humour and whim" of dogs; and describes their occasional singular *antipathies* to man or other animals, which are comparable with the sudden, unaccountable, and irresistible dislikes occasionally exhibited by man. He cites an example of eccentricity and exceptional stupidity in his dog Hector, which he describes as barking round the room "like a crazed beast"—recognising, apparently, by such an expression, the fact that "beasts" are apt to become "*crazed*"—that is, mentally deranged. The newspapers frequently contain records of the "*singular conduct*" of various animals. Thus, I read quite recently,³ under the heading "Singular Conduct of a Dog," a paragraph of which the following were the leading features. A large bull-dog, "singularly ferocious in appearance," a watch-dog in a rope work near Greenock, suddenly rushed into a house, which was quite unknown to it, and which it had never before entered. It ensconced itself on a certain bed in a certain room; which bed "coaxing, threats, and food had no effect in inducing it to leave." It literally showed its teeth to the alarmed inmates of the house and the neighbours, who

¹ "My tame Peregrine Falcon," says Mr. St. John, in his 'Natural History and Sport in Morayshire,' "after some years of perfect friendship and alliance with my pet owl, ended in *killing and eating her*!"

² Stupidité in man, according to various French authors (*e.g.* Baillarger), is a form of acute dementia or melancholia.

³ 'Glasgow Daily Herald,' November 8, 1870.

were strangers to it, but who had, however, treated it only with kindness. But it "at once leapt from the bed, and began exhibiting as great evidence of kindness as it had previously shown of ferocity," when approached by one of its master's workmen, whom it recognised. This is not a very striking illustration of eccentricity, as reported by the press, but there was probably some suspicion at least of insanity to account for its attracting the notice of a newspaper correspondent at all during these war times. Dr. Bennett describes the Siamang as exhibiting "all the freaks of temper of a spoilt child." The *eccentricities* of animals are not, however, all unaccountable. Inquiry, in some cases at least, has traced their causation to *mental shock*, such as fright, or to *bad usage* by man; and it is, at all events, probable that many others would, on investigation, be traced to some similar source. Jesse describes the dog as sometimes becoming "*frantic*" without apparent or adequate cause; in illustration of which he mentions the case of one being invariably excited to *frenzy* by the sight of a four-wheeled carriage (p. 51). He speaks of other "*odd peculiarities*" (p. 283), adducing as examples the London fireman's dog (p. 56); and a canine "Paul Pry," which exhibited extraordinary curiosity (p. 289). The author of the "Anecdotes of Dogs," in 'Chambers's Miscellany,' gives a section on "*Eccentricities in Dogs*" (p. 30).

The question here naturally interjects itself: What is the so-called *madness* of animals as contra-distinguished from *insanity* in man? How are both to be defined, if they are definable? To what extent, or in what sense, is *animal madness* synonymous with *human insanity*?

It would appear that the term *madness*, as applied to animals, is usually supposed to refer to *rabies*,¹ a specific contagious disease,² that is sometimes spontaneously developed in the dog, wolf, and other animals, and which is transmissible not only to other animals of different species, but to man, in him constituting *hydrophobia*.³ According to Professor Dick, the *form* of madness developed depends on the portion of the mucous membrane affected by the specific inflammation (p. 375). He speaks of two prominent forms of rabietic⁴ madness, viz. "dumb" or "still" madness, as occurring in the dog (pp.

¹ There is no need whatever for *two* technical terms to designate what is essentially *the same* disease in man and other animals.

² Gamgee, p. 450.

³ Prof. Dick did not believe in *human* hydrophobia.

⁴ I avoid the use of the term *rabid*, which frequently means simply irritable, fiery, or ferocious. Thus I have seen the term "*rabid temper*" applied to the horse.

354, 366); and of "raging" or "furious" madness in a mare (p. 357). Mental excitement and delirium are, in his opinion, only common, not necessary, symptoms or accompaniments; and, when they occur, they depend on the extension of the inflammation from the nasal mucous membrane to the brain and its membranes. Among the *prodromata* of rabies there is frequently, according to Gamgee, a notable *change in the affections*, character, or habits of the dog. In human hydrophobia there are usually decided *mental* symptoms in the last stage, including irritability, apprehensions or suspicions, sometimes *delirium*, occasionally *mania*, the delirium and mania passing into each other, or not properly separable, or the one apparently being substituted for the other. Dr. Bettelheim, of Vienna, has quite recently described the mental excitement that characterises hydrophobia in children. He speaks of "*emotional* derangement—the psychosis, which, in the further progress of typical cases, forms the mid point of the symptoms;" "frightful dreams," and "gloomy excitement. They fall a prey to restlessness, which drives them aimlessly hither and thither. This excitement becomes more and more intense, till it becomes a fearful and indescribable agony. . . . The movements become violent and dangerous, and the speech loud and rapid. The distress is often concentrated into frightful paroxysms, especially in children, when anything unusual occurs; and the patient frequently exhibits the appearance of a person on the verge of a maniacal outbreak, the behaviour being wild and dejected, the countenance flushed, and the eyes restless and flashing. . . The mental irritation may amount to a real *maniacal* seizure, and this occurs all the sooner where restraint by manacles and such like is resorted to. Indeed, they bite also, as do all raving persons, to whom no other means of defence is at hand. But there are also *maniacal* attacks, which occur without any external stimulus, and in the intervals of which the patient himself is aware of their approach, and warns his neighbours. Sometimes *delirium* occurs, but generally of a kind out of which the patient may be easily brought to himself."¹ Now, a precisely similar series of *mental* phenomena occurs in the rabietic dog; and if, therefore, it is proper to apply the term *mania* or *insanity* to the mental condition above described in man, it must be equally right to do so in the case of the dog. Dr. Bettelheim speaks of the "*stadium melancholicum* of Swieten, which corresponds to a *completely analogous condition in dogs*—a condition of unrest and of going hither and thither,

¹ On "Hydrophobia," 'Journal of Mental Science,' April, 1870, pp. 100—102.

with simultaneous depression. The animals are still obedient; they recognise their masters; but they are irritated, and will even bite if they are played with" (p. 101). In a subsequent stage "the dogs have *unmistakable delusions and hallucinations*;¹ and according to their temperament and race are affected by more or less *maniacal attacks*" (p. 102). Such are the recent statements of the clinical assistant of one of the most distinguished living physicians of Vienna, Professor Oppolzer. How Dr. Bettelheim satisfies himself of the existence of "*unmistakable delusions and hallucinations*," he does not, however, explain. Even in man it is frequently difficult or impossible to *prove* the existence of delusion, hallucination or illusion! The disease known as *rabies* in animals and *hydrophobia* in man does not itself, however, belong to the category of *insanity*, which is defined (though erroneously) to be a disease *without fever*. It is to be classed rather with the fevers, or general disorders, produced by blood-poisoning.² But in so far as *mania* is a "disorder of the *intellect*,"³ according to the latest "Nomenclature of diseases," that of the Royal College of Physicians (1869), there can be little doubt that *insanity* occurs equally in the *rabies* of animals and the *hydrophobia* of man. And, further, *mania*, and other unquestionable "disorders of the *intellect*," are developed in *many* morbid conditions of the lower animals. Mental derangement, however, equally in animals and man, may either be part of a major disease, a mere symptom of a general disorder; or it may itself constitute the major disease. Unfortunately an artificial distinction has been created according as it is merely symptomatic or not; and this distinction has been the cause of great confusion in the study of insanity,

¹ The differential definition of *delusion*, *hallucination*, and *illusion*, as given by authors, is most arbitrary and confusing, and their distinction mischievous. Unquestionably they pass into each other even in the same individual. Not only are these terms used synonymously by the best writers, but they are employed frequently in most improper senses; an illustration whereof may be found (as regards "hallucination" and "illusion") in Professor Aitken's 'Practice of Physic' (pp. 403, 416, 424, 425, 430, 431). It appears to me preferable to use the single term *delusion*, specifying impairment of the *senses* where it occurs. In the said work of Professor Aitken's, "delusion" is so defined (p. 417) as to include "hallucination" and "illusion." Forbes Winslow regards them as in certain cases synonymous; while Tuke also recognises apparently the difficulty of distinguishing them properly ('Manual,' pp. 127—129).

² It is one of the anomalies of the Royal College of Physicians' Nosology, that while glanders, farcy, equinia, and malignant pustule, are all classed, with fevers, among "General Diseases," *hydrophobia* is placed among "functional diseases of the nervous system;" *hydrophobia* and *rabies*, however, being both associated with glanders, farcy, equinia, and malignant pustule, also in the category of "poisoned wounds."

³ This is an error. It is only so occasionally. In other cases it is a disorder of the *emotions* only. Professor Aitken describes it as essentially a disorder of the *impulses, propensities, or passions*, in the first instance.

even in man. In the human subject we separate from insanity the *delirium* of fever, of various acute inflammations, of starvation, of intoxication, of the action of certain poisons, as well as the *delusions*¹ of *delirium tremens*. But such a distinction is purely arbitrary and artificial.² *Delirium* and *mania* are merely stages of what is essentially the same kind of mental disorder;³ and it is utterly absurd to separate them on the sole ground of the presence in the one case of obvious inflammation or *fever*, and its apparent absence in the other.⁴ The use of the thermometer would show probably that, in the so-called insanity of man, "elevation of temperature," which constitutes the essence of fever,⁵ is more common than is usually supposed. But fever itself is merely a "complex morbid state, which accompanies many diseases as part of their phenomena;"⁵ and its introduction into any definition of insanity is most improper. The definitions of human insanity given by our best modern authors⁶ are *eminently unsatisfactory*: but whichever one be adopted as a standard, it will be found to include much at least of the madness of the lower animals. In truth, however, equally as regards man and animals,

"To define true madness,
What is't but to be nothing else but mad?"

If the term *insanity* is a vague and indefinable one in man,⁷ the word *madness* is still more so as applied to animals. It is obvious that many cases of animal madness cannot be attributed to rabies; and veterinarians themselves, by the terms they employ, appear to recognise in animals other forms of insanity than the *furiosity* or *mania* of rabies. Thus they speak variously of *vice*, *frenzy*, *fury*, *craziness*, *crankiness*, *franticness*, as something different from *rabidity*. Each, however, has his own interpretation or application of these terms, and the general designation *madness* is applied to *conditions of the most diverse kinds*. Principal Williams, of the Edinburgh Veterinary College,

¹ *Vide* foot-note¹ on preceding page.

² *Vide* Bucknill and Tuke's 'Manual,' p. 357.

³ Thus Professor Dick describes *delirium* and *furiosity* (or *madness*) in the same *marc* (p. 358). Many similar instances have been recorded as occurring in man, *e.g.*, in Bucknill and Tuke's 'Manual,' p. 120.

⁴ "Without pyrexia" (Cullen); "without fever" (Aretæus); "apyrexial" (Guislain). *Vide* Bucknill and Tuke's 'Manual,' pp. 77, 78, 80—82, 313, 377.

⁵ 'The Science and Practice of Medicine,' by Prof. Aitken, 4th ed. (1866), vol. i, p. 29.

⁶ *Ibid.*, vol. ii, p. 402; or Bucknill and Tuke's 'Manual of Psychological Medicine,' pp. 76—78, and 269 ("Diagnosis of Insanity").

⁷ "Insanity may be occasioned by any and every *pathological* state, which is capable of taking place within the substance of the *brain*." (Bucknill; 'Manual,' p. 358.)

wrote me, in November, 1869—"During my experience as a veterinary surgeon, I have seen many cases of *acute madness* (if I may so term it) from inflammation of the brain and its membranes, arising idiopathically, traumatically, or from *sympathy*,¹ as in engorgement of the stomach. These are cases of short duration, and terminate favorably or fatally in a few days. I have also seen *intermitting madness* from various causes—tumours on brain, thickening of the meninges, abscesses, &c. I saw a case about a month ago where there was *intermitting frenzy* from softening of the cranial bones. I have the specimen, the horse having been destroyed. I have also seen a case of what I thought the nearest approach to *idiocy*² in a horse, supervening upon the formation of abscesses. This animal remained an *idiot* for many years. He was called a 'cranky' horse, but was harmless and did his work well enough.³ In all cases there has been structural alteration in the cranium, if we except that arising from gorged stomach." Nevertheless, he adds, "I do not think that they [the lower animals] have a *mind* to be affected by the causes that produce insanity in man!"

Professor McBride, of the Royal Agricultural College, Cirencester, wrote me, in April, 1870, "I do not remember to have seen any cases recorded of *madness* in the lower animals, except those of *frenzy*, resulting from blood-disorders, or diseases of the digestive organs. There are certain forms of excitement, commonly called *vice*, which I firmly believe to be *insanity*, and which is often seen to be *hereditary*. The whole subject is one that veterinarians have not given much attention to; but it is of such an interesting nature, that I shall be most happy to receive any hints from you as to the best mode of investigating it."

In the newspapers, especially during summer, one frequently

¹ Insanity by *sympathy* (e.g. with affections of or involving peripheral portions of the nervous system) is common in man, and is at least likely to be equally so in other animals. It is most liable to occur where "constitutional irritability" exists; that is, where "slight causes of nervous action produce great effects" (Bucknill and Tuke's 'Manual,' p. 394). It is by this powerful *law of sympathy*, that the irritation or injury of distant organs so frequently leads to functional derangement of the brain; that insanity so commonly follows chronic *dysepsia* in man, and probably also in other animals.

² The term *idiocy* (= *Amentia*), as applied to man, refers to *congenital* defective development of brain and mind; while mental impairment supervening *subsequently to birth*, as the result (as in this horse) of disease, or of age, accident, or injury, is known as *dementia*. Popular or scientific distinctions between *dementia* and *imbecility* or *fatuity* (which are merely *different degrees of the same condition*) are, as in the case of delusion, hallucination, and illusion, both unnecessary and mischievous.

³ The case is apparently analogous to the harmless industrious *dementia* so common among lunatic paupers.

meets with notices of "*mad*" animals of various kinds running loose in the streets, to the terror of the inhabitants; or of their being the cause of dangerous bites or other serious accidents to human life. Some of these instances of so-called *madness* may really be rabies; but in many other cases there is no evidence of the existence of that disease, and the furiosity is then probably attributable to suddenly developed and frequently ephemeral *mania*. "Give a dog a bad name" has become a proverb; and there can be no doubt that if a dog is considered, rightly or wrongly, "*mad*," and it is subjected to the human pursuit and brutality that are so common when a wretched animal does become possessed of such a reputation, a dangerous degree of mental excitement is easily provoked, which may amount to, or pass into, *mania*. Professor Dick speaks of *furiosity* in the ox (p. 362), apparently as attributable to rabies. Occasionally the public prints record the dangerous *eccentricities* of "an infuriated ox," its running-a-muck, or butting at and upsetting all who come in its way. This kind of furiosity is quite as likely to be *mania* as rabies. A recent New Zealand newspaper¹ relates that in the streets of Auckland a *mad pig* "bit a lady and several boys, and tore the clothes of a man whom it knocked down." A still more recent New York paper² cites the following case of a bloodhound mastiff, near New Brighton, Staten Island, N.Y. The animal was a "rare and costly" one, of a very large and fine breed, fully three feet high. It had long been "an object of terror to every one," though for what reason does not appear, inasmuch as it is described as having been "of a docile temperament." It was not, however, usually allowed liberty; but on a certain Sunday it was at large, reclining quietly on the lawn in front of the mansion to which it belonged, in company with a Newfoundland bitch, belonging to a neighbouring resident. Near the animals some of the female servants were squatted, lazily sunning themselves, in the absence of their master and mistress at church. "Suddenly, without any warning or provocation," the hound made a rush at one of the girls, who at once took to flight. "He betrayed the fiercest desperation, and with lowered head, protruding tongue, and flashing eyes, darted after her." Overtaking her, he "buried his long, fang-like teeth in her shoulder and back. Both rolled over, the dog snarling and biting." Another girl, having gone to her friend's assistance, "the hound sprang upon her, and bit her severely . . . About twenty wounds were found upon the arm, breast, and thigh" of one, and "several upon the arms and thighs" of the other, girl. "Both girls are in a precarious condition. Bessie presented a terribly mangled

¹ 'Otago Daily Times,' July 30, 1870.

² September, 1870.

appearance." A surgeon "treated these wounds as *rabid bites*, although it is *not certain that they are*." The animal was at once shot, the usual *treatment of mania* and all kinds of mental affections in animals—a treatment which is based on utter ignorance of the natural history of animal insanity!

While the *madness* of animals includes many phenomena that belong to the category of *insanity* as it exists in man, there are cases of so-called insanity or *lunacy* in animals that may not be referable to that category. Thus Dr. Kane described his team of Esquimo dogs as suffering from a "disease which he considered *clearly mental* . . . An *epileptic* attack was followed by *true lunacy*."¹ Now, Dr. Robert Brown informs me² that in his opinion the fatal disease alluded to by Kane is "only the contagious disease, which has for the last few years been decimating the native dogs of Greenland, the western shores of Davis's Straits, and Kamtschatka."

According to the latest Nosology—that already quoted of the Royal College of Physicians—the *chief forms of insanity* in man are—

- | | |
|-----------------------------|------------------------|
| 1. Mania, | } including Monomania. |
| 2. Melancholia, | |
| 3. Dementia. | |
| 4. General paralysis. | |
| 5. Idiocy. | |
| 6. Imbecility. ³ | |

This is a most defective classification, embracing only "disorders of the *intellect*," and omitting, as it does, all separate disorders of the *instincts, emotions, passions, propensities*,⁴ *volition*, or self-control, belonging to the important, though somewhat heterogeneous, category of *moral* or *emotional* insanity. On the other hand, its simplicity renders it preferable, for practical purposes, to the other more elaborate, confusing, and equally unscientific, classifications of modern times.⁵ Accepting, however, the College of Physicians' classification as adequate for our present purpose, there is, I think, sufficient ground for believing that the lower animals are subject to all the forms of insanity enumerated in that nosology, with the single exception, perhaps, of general

¹ *I* *vide* my paper on "The Causes of Insanity in Arctic Countries," p. 216 (formerly quoted).

² January, 1870. *I* *vide* his paper on "The Mammalian Fauna of Greenland," 'Proceedings of Zoological Society,' 1868.

³ 'The Nomenclature of Diseases,' 1869, p. 41.

⁴ Bucknill and Tuke refer mania to *insanity of the propensities*—klepto-, eroto-, pyro-, and dipso-mania—homicidal and suicidal.

⁵ I have pointed out some of the *absurdities of modern classifications* in papers on (1) "Temporary Insanity," 'Edinburgh Medical Journal,' vol. xi (1865), p. 445. (2) "Typhomania," *ibid.*, vol. xiv. (1868), p. 331.

paralysis. And I have a strong conviction that this disease also will be found among them *when it is duly searched for*. In all probability it is at present confounded with and mistaken for a form of ordinary “palsy,” spinal or cerebro-spinal.

I. *Mania*.—The evidences of the existence of mania in animals are comparatively numerous and convincing. Several instances have already been given from competent authorities, and many others might be cited. Dr. John Brown wrote me in December, 1869, “I have heard Professor Dick describe, with his coarse power, the delirium and *maniacal* excitement of a horse.” Percival, too, describes the horse as *mad*—“furiously so in the worst sense of the word”—in the staggers¹ (phrenitis). Hartwig speaks of *ungovernable fury*,² approaching at least the character of mania, developed without external cause, as not uncommon among certain wild tropical animals, when subjected to *captivity* or unnatural *solitude*. Thus, he says the black rhinoceros not seldom falls into *paroxysms of rage* without any evident cause. It is very *furiously* when provoked or attacked, and “when roused to passion there is nothing more terrific on earth!” Other writers speak of the fierce or violent *temper* of the rhinoceros and chamois. The “rogue” (“bachelor” or “outcast”) hippopotamus sometimes runs-a-muck, attacking every person or animal that approaches it. Hence it is extremely dangerous. The same phenomena are exhibited by the sperm whale, sea bear and elephant under similar circumstances.³ Indian officers speak of “*insane*” elephants. Chambers alludes to their “*paroxysms of madness*,” and “*periodical fits of rage*.” Various writers refer to the fury or passion of both the elephant and camel as sometimes fatal to man. In the latter animal Palgrave ascribes it to “forethoughted malice;” while other authors assign to it a notoriously bad temper. Chambers speaks of violent *fits of rage* occasionally occurring in tamed, young, Asiatic horses.⁴ Menault describes the cat as exhibiting great excitement or fury under unusual circumstances. In some of these cases the temper-excitement may amount only to *passion*;⁵ but it is impossible to define or determine where passion merges into *mania*. In certain cases the temper may be

¹ Gamgee, p. 482.

² *Emphathema*—*ungovernable passion*—was one of the *forms* of insanity according to Dr. Mason Good’s Nosology. It was also the “*emportement maniaque*” of Pinel:—an *instinctive fury* excited by the least provocation (Bucknill and Tuke’s ‘Manual,’ p. 189).

³ ‘The Tropical World,’ by Dr. Hartwig, 1863, p. 443.

⁴ Article *Horse*, ‘Encyclopædia,’ p. 3.

⁵ Paroxysms of blind fury, we have seen, occur also in man; and in him are regarded as manifestations of disordered brain and mind—being referred to the category of *emotional* (= *impulsive, instinctive, or volitional*) insanity.

naturally passionate. More frequently, it is to be feared, it is rendered "ferocious" by various kinds of provocation, some of them unavoidable (in the case of animals in a state of nature), but others unquestionably avoidable (in the case of ill-usage by man). It has already been mentioned that maniacal or other passion in animals leads them sometimes to *murder* each other. This cannot be called, but it is apparently the equivalent of, *homicidal mania* in man.

2. *Melancholia* would appear to be as common as mania. Several cases have already been cited of inordinate *grief* amounting or leading to melancholia. The *friendships* of animals for each other or for man are sometimes of such a character that one companion is incapable of surviving the death of another. *Grief* at the loss of masters is also frequently so extreme as to lead to apathy, abstinence, melancholia and *death*. Such deaths have been described as virtually *suicides*. Thus Menault says of such a case in a horse, "or rather, as we may justly say, *committed suicide from grief* at the loss of an old friend" (p. 281). No doubt, in such cases, there is *self-starvation*; but the *deliberation* implied in suicide is far from proven. All we can venture to assert is that mental shock leads to melancholia with abstinence,¹ and that the abstinence and mental depression, combined with the general shock to the nervous system, lead to death. In some instances, suicidal deliberation may have been present. Thus, Jesse records what he calls two *suicides by starvation* in the dog from grief at the loss of masters (pp. 305 and 306); and from jealousy of attentions shown to a child, to a wife, or to another dog (p. 269). Menault gives various instances of *self-immolation* in dogs—of their sacrificing themselves to death on the funeral piles of their masters (p. 332). Captain Hutchinson, R.A., in his work on 'Lapland' (1870, p. 166), relates the following anecdote: "Our Lapp dog, tired of the *monotony* of the long journey, sought to put an end to it by *suicide*. He jumped out and hung inside the wheel by his rope, which was fastened to our gig. He was very nearly strangled before we could release him. The facility with which we could stop the pony at the word *Phu-r-r-r* no doubt saved his life." There is no proof so far that suicide was intended. It might have been a mere accident, a jumping out ignorant of the consequences. But that suicidal intention was present is rendered at least probable by the sequel; for it is of the same dog (apparently) that he further writes (p. 172): "Feeling how impossible it would be to survive our departure, he employed the first night in prepar-

¹ As of Dryden's typical melancholiac, it may be said of such a dog,

"He eats not, drinks not, sleeps not. . . ."

ing himself a *grave*, in which he was found the next morning with nothing but the tip of his tail appearing above ground!" Much more decided, however, was the following narrative, headed "*Suicide by a Dog*," which occurred recently in the Irish and Scotch newspapers.¹ "The 'Cork Examiner' tells a long tale of *self-murder* by a dog attached to the Cunard tender, and well known in Queenstown by the name of 'Ney.' Divested of an amatory episode and other irrelevant circumstances, the facts of the case, vouched for by an eye-witness, one of the Cunard Company's officers, are these: Early on Friday morning he fled to the beach, and howling a farewell to all his friends, threw himself headlong into the sea. Several spectators rushed into the water and attempted to drag him out. But the animal, *deliberately* evading every grasp, plunged again and again beneath the water, through which he was observed *clutching the bottom* with his paws as if in *desperate endeavour* to hurry his death; while more than once, on coming, despite his struggles, to the surface, he *snapped savagely* at the hands outstretched to save him. The men, thinking the dog *mad*, at length left him to his fate, which he soon achieved, and sank to rise no more." Jesse also relates two cases of *determined suicide* by drowning in the dog, preceded by want of its usual animation, and a condition analogous or equivalent to *melancholia* (p. 145). If, as Davey asserts,² *all* suicide in man is the result of insanity—an opinion in which I am disposed to concur—it must be regarded as equally so in the lower animals. So long ago as 1804, this view—*quoad* man—was clearly set forth by Dr. Rowley, in a little work 'On Madness and Suicide,' wherein he says, "Suicide should ever be considered an *act of insanity*." The same opinion is generally adopted and acted upon throughout the United States; and it must be regarded as an illustration of the superior enlightenment of our Transatlantic cousins that it is so. The 'Philadelphia Medical and Surgical Reporter' has stated quite recently,³ that in America "the idea of *crime* is no longer connected with such attempts. The universal presumption is, that the person who attempts self-murder is *insane*, and therefore not amenable to judicial inquiry. . . . Suicide, as a crime, is no longer recognised in this country, and the term ceases to be applicable. No penalty is inflicted for the attempt, and posthumous proceedings against the estate are unknown in America."⁴ But as in the case last narrated, the act of suicide

¹ 'Edinburgh Evening Courant,' June 16, 1870.

² On "Felo-de-se," 'Journal of Mental Science,' October, 1870, p. 390.

³ *Ibid* 'Edinburgh Medical Journal,' November, 1870, p. 477.

⁴ Compare with this procedure and state of public opinion the practice of our own colonies, as illustrated by a case cited in my paper on "Insanity in the

in dogs is, at least usually, preceded by, or associated with, *other* mental phenomena attributable to the category of insanity. Hartwig describes the black rhinoceros as of a "gloomy, melancholy¹ temper" (p. 443); while Jesse also speaks of the dog having occasionally a "melancholy" or "anxious" expression of countenance. Here again, however, must be borne in mind the distinction between a temper that is naturally morose, and one that is rendered so by disease or irritation; as well as between temper and insanity. *Nostalgia* is quite likely to occur in domesticated animals having strong attachments to locality, *e.g.* the dog and cat.

3. *Delusional insanity*.—In the absence of the power of *speech*, it may be impossible to *prove* the existence of the faculty of imagination, or of delusions, illusions, and hallucinations, in animals. Nevertheless, there is good ground for believing that the said faculty, and the said perversions of the intellect or the senses, really occur. Professor Goodsir, who held that man has "a mind higher than and entirely distinct from the *instinctive* consciousness of the animal" (p. 222), nevertheless admits that "the brute is *undoubtedly capable of memory and imagination*" (p. 311). Lord Brougham² relates that dogs *dream*, apparently just as we do: while Sir Alexander Morison³ says that some of the lower animals, especially the dog, "appear to be subject" to *nightmare*, which implies dreaming; and dreaming involves the play of fancy, the operation of the *imagination*. In certain wild animals, especially those which are much hunted by man, his stratagems and snares beget a morbid cautiousness that passes into suspiciousness and timidity; while gradually *delusions of suspicion or fear* appear sometimes to be developed. Thus, Menault says of the wolf, that a morbid timidity is apt to be engendered, sometimes giving rise to "*idle fancies* and false decisions, which are the fruit of *imagination*;" that is to say, delusions, illusions, or hallucinations are created just as in man. "If these false opinions extend to a certain number of objects, he will become the plaything of *illusions*,⁴ which will precipitate him into an infinity of false proceedings. He will see traps where there are none. *Fright*, deranging his *imagination*, will

British Colonies," 'Brit. and Foreign Medico-Chirurgical Review,' vol. xlv (1869), p. 485.

¹ Whole nations have been described as of a "melancholy disposition," *e.g.* the Icelanders (*Vide* paper on "Insanity in Arctic Countries," p. 212). Bucknill and Tuke recognise a man "constitutionally sad and morose" as being nevertheless quite sane.

² Lord Brougham's works, vol. vi, 1856; "Dialogues on Instinct," chapters on 'Animal Intelligence.'

³ 'Outlines of Mental Diseases,' 3rd ed., 1829, p. 124.

⁴ "*Illusions*" are here confounded with "*Hallucinations*" (*Vide* foot-note ¹, p. 46).

represent to him in another order the different feelings which he has received, and will form out of them deceiving forms to which he will attach an *abstract idea*¹ of peril. This, in fact, is noticed in carnivorous animals everywhere, when they are often hunted and continually besieged with traps" (p. 308). Dr. Bettelheim's opinion regarding delusions and hallucinations in the dog has already been recorded. Naturalists tell us of *singular impressions* that are sometimes made in the mind of the *pregnant* bitch, and which are liable to affect parturition, or the offspring, just as in man.² (e. g. Jesse, pp. 75 and 408.) Some of these at least imply disorder of the *imaginative* faculty.

4. *Amentia*³—original deficiency, or imperfect development, of the intellectual faculties, and *dementia*⁴—their disorder subsequent to birth, are probably both to be found among the lower animals, though the evidences of their existence are less numerous and obvious than in the case of mania and melancholia. The various grades of *imbecility* and *fatuity*⁵ are probably accompaniments of *age*, as well as of cerebral disease, just as in man. They include, perhaps, many of the cases referred by veterinarians to *craziness* or *crankiness*. What they call *idiocy* is also probably *dementia*, rather than *amentia*, inasmuch as it is usually developed *subsequent to birth* in animals for a time really or apparently sane.⁶

5. *Moral insanity*.⁷—Equally in man and animals there are *many* forms of insanity that do not involve manifest *intellectual* disorder, but include derangements of the *emotions* or *feelings*, *passions*, *propensities*, *volition*, *moral sense*, or *instincts*.⁸ Though

¹ Bishop Berkeley, in his 'Introduction' to the 'Principles of Human Knowledge' (Sect. XI), says, commenting on Locke's views as to *animal reason*, "I readily agree with this learned author that the faculties of brutes can by no means attain to *abstraction*. But then, if *this* be made the distinguishing property of that sort of animals, I fear a great many of those that *pass for men* must be reckoned into their number!"

² Moreover, a constant state of alarm or anxiety during *pregnancy* is as likely to lead, in other animals as in man, to the production of young subject to convulsive or nervous diseases, or to forms of *insanity*, especially *dementia*.

³ *Ibid* foot-note 2, p. 48.

⁴ *Ibid*.

⁵ *Ibid*.

⁶ *Ibid*.

⁷ See its definition by Pritchard in Bucknill and Tuke's 'Manual,' pp. 154, 190, and 328. It is the "*emotional insanity*" of the last-named authors.

⁸ Alienists recognise *instinctive* insanity in man. Thus, Marc says, "It is necessary to admit, since facts demand it, that there are two sorts of monomania, one of which is *instinctive*, the other reasoning. The first bears the monomaniac on by the effort of his *will*—primarily diseased—to *instinctive* and automatic acts, which are not preceded by reasoning; the other determines acts which are the consequence of a certain association of ideas" (vol. i, p. 244). Esquirol, too, divided monomania partly into *affective* and *instinctive*. In the first case the affections and dispositions are perverted. In the second, "the actions are involuntary, *instinctive*, irresistible." (*Vide* Bucknill and Tuke's 'Manual of Psychological Medicine,' p. 189, where it is shown that Pinel also admitted an

agreeing in the non-involvement of the intellectual faculties proper of reason, thought, or judgment, these other phases of insanity form a somewhat heterogeneous group, for which the designation *moral insanity* is only partly appropriate, and which have been partly also arranged by nosologists under the heads of *emotional, affective, impulsive, instinctive, or volitional insanity*. To this category belong some forms of *mania* and *melancholia*; for instance, those in which the *propensity to murder or suicide* exists without the presence of delusion or intellectual disturbance. Self-preservation or love of life is an *instinct* both in man and animals, and equally in both it is disordered or abrogated in *suicide*.¹ Love of offspring—maternal affection—is another instinct equally in man and animals. In man it is deranged, where, as in the puerperal state, the mother sacrifices her child; while in animals the same species of *murder* is occasionally exhibited as a phenomenon distinct from and unconnected with the gradual alienation of affection for their young that is common in many of the lower animals as a concomitant of maturation. And, lastly, disorder of the *sexual instinct* is illustrated by *Erotomania*, which may be held to include the so-called *satyriasis* (of the male), and *nymphomania* (of the female). Both in man and other animals the instinct in question is liable to extreme excitement from pathological changes in the nervous system.

I have little doubt that much of what is called *instinct* in animals is the exact equivalent of what is known as *reason* in man, and has quite as good a title to be so designated; while, on the other hand, much that is called reason in man is the precise counterpart of instinct in animals, and has equal claim to be so considered and denominated. Little, however, is known—of a character that has any scientific value—of human instinct; and in truth Instinct, whether applied to animals or man, is a term that has hitherto served as a cloak for ignorance, and has been a serious obstacle to all inquiry. It ought undoubtedly to be disused, unless in its rigid application to what is really *primitive*, original, or innate in the mind, as contrasted with knowledge that has been *acquired* by experience and education.

"*instinctive madness*;" while Dr. Tuke himself speaks of "*instinctive fury*." See pp. 78, 79, 191, 197, 201, 207.)

¹ "Perversion of the natural *instinct* of self-preservation is undoubtedly the immediate cause in one class of cases; and it may be said that in all, this conservative principle, so deeply implanted in the inner recesses of our constitution, is overborne, if not itself primarily at fault, by the diseased action of other mental faculties or *instincts*. . . ." "There can be no doubt that it is capable of being *pathologically affected strictly by itself*. . . . The desire to die by one's own act appears to be the one mental symptom, and to present the most undoubted instance of disease affecting *only one function*"—that is *true monomania*. (Bucknill and Tuke, pp. 201 and 327.)

There is not, perhaps, in the whole science of *comparative psychology*, a study that is more interesting than the much misunderstood subject of *instinct as contradistinguished from reason in man as well as in the lower animals*!

The category of *moral insanity* includes probably many of the cases that have been already mentioned of remarkable *changes in the habits or disposition* of animals. *Erotomania*, as we have seen, is most likely to occur among them; but we are much less prepared to recognise the existence of *kleptomania*. Jesse tells us that dogs can be educated to *steal*, knowing well in some cases that they are stealing (p. 316); and this is only one of the many evils, which accrue to the lower animals from their intimate association with man! He represents them as becoming "shy, sullen, and sheepish," when charged with *theft*, showing their possession of "a kind of *moral sense*"¹ (p. 47). So obvious is the sense of shame, disgrace, or discomfiture—of detection in wrong-doing—that, "slinking away with its tail between its legs," like a dog, has become a proverbial term for the expression of a self-feeling of merited disgrace. Jesse speaks of sheep-stealing dogs being *incapable of honesty*; and he cites the once well-known case of "Yarrow" (p. 222). It is difficult, however, to distinguish in such cases the mere force of habit or vice, which is the result of man's evil training and example, from such perversion of the moral sense as amounts to *disease*, placing the propensity beyond the regulation of will or self-control. The raven, we are told, is a dexterous *thief*, frequently stealing for the mere *pleasure afforded by the act*.² The magpie, too, is noted for theft, exhibiting the most ingenious tricks for the accomplishment of its end.³ Here, however, as in certain other cases, the propensity to steal is perhaps to be regarded as natural to the animal.

Cats are described as liable to a malady, which "manifests itself by an irrestrainable *propensity to run away*;" the equivalent of which in man is the *drapetomania* of the negro. (*Vide* 'Bucknill and Tuke's Manual,' p. 48.)

¹ "Many animals have all—all animals have some—of the passionate elements of *morality*. If a dog could *speak*, he could be taught a good deal of morality.... He knows when he has done *wrong* and deserves to be flogged, and he views *deserved punishment* quite differently from wanton cruelty. How far this goes it is impossible to say, by reason of *our* lack of sympathy with animal understanding." . . . And yet "To many persons the *moral* distinction between man and brutes may appear broader than the intellectual one." . . . The dog "must have an indistinct notion of *principles* of conduct, in some degree analogous to *moral obligation*." . . . His "affection for his master is very like *piety*." . . . He has a perception of *duty*. . . . Every dog *knows* when he has done *wrong*. So, too, he is capable of distinguishing between a wrong and a hurt, which is not a wrong—at least, where the contrast is glaring."—'Saturday Review,' July 30, 1864.

² 'Cassell's Brehm's Book of Birds,' edited by Professor Rymer Jones.

³ Menault.

There are some eminent authors who hold animals *irresponsible* for their acts, *irrespective of insanity*. Thus, Professor Goodsir writes that animals have "no field . . . for the exercise of judgment, and can, therefore, commit no error, nor be responsible for any act" (p. 327). If this assertion implies, as it apparently does, a denial that animals possess reason or judgment, or have an understanding of the nature of right and wrong, of reward and punishment, it is obviously and utterly opposed to multitudinous observations on animal habits.

I do not doubt that I might have made large and important additions to these Illustrations of Eccentricity and Insanity in the Lower animals, had I had opportunity of consulting *foreign* works on veterinary science, or on animal habits and disorders; for all these subjects have attracted much more attention, and have been more fully studied, on the continent, where also the veterinary schools and colleges are greatly superior to our own.¹ According to 'Nature,' a "Course of Lectures" was in the spring of this year (1870) delivered in Paris on "Madness in Man and Animals," by Professor Bouley, Inspector-General of Veterinary Schools in France. I have not seen the work itself, and the present is not a favorable time for communicating by letter with Paris.² But such notices as I have seen in 'Nature,' or other British journals, refer exclusively to rabies or hydrophobia.

In all likelihood the facts and inferences above recorded will be regarded, by a proportion at least of my readers, as pointing only to a *probability* that the lower animals are subject, like man, to insanity. I am quite willing to accept this conclusion, though I think the evidence already accumulated warrants us in going much further than the admission of mere probability. But, assuming that only a probability yet exists, I submit that the testimony already adducible is of such a character as to render worth while immediate search for the *proofs*, and indi-

¹ In France, for instance, there are three celebrated "Écoles Impériales Vétérinaires"—at Alfort, Lyons, and Toulon. Of these, the first named is the most important—situated about three miles from *Paris*, near Charenton, accessible by rail or bus. It contains a botanic garden, museum, library, chemical laboratory, pharmacy, dissecting room, shoeing forge, class rooms, infirmary for horses, kennel for dogs under treatment, pig sty, and sheep fold, with specimens of most of the known breeds of sheep. The modern school of *Alfort* is a development of the old veterinary school of Paris. In some of the Continental *Universities*, moreover, there are Professorships of Veterinary Science. For instance, in that of Prague there is a chair of "Theoretical and Practical Veterinary Medicine and Veterinary Police;" while in that of Göttingen Dr. Luefling is Professor of the "Anatomy, Physiology, and Pathology of the Domestic Animals."

² While I write (October and November, 1870) Paris is invested by the Prussians; is daily looking for bombardment; and all letter communication is by balloons

cates the nature of the further evidence that may be expected to be forthcoming *when duly looked for*. I therefore venture earnestly to recommend the subject to the attention equally of the physician and the veterinarian, of all who come much in contact with the lower, and especially the domesticated, animals, in order that more, and more authentic, materials may be collected towards a knowledge of the phenomena of *Animal Insanity*, of a fullness and accuracy commensurate with its importance.

I have only to add, in conclusion, that I know of no department of Psychology, which, at present, promises so rich a reward to the investigator as that relating to the "*Physiology and Pathology of Mind in the Lower Animals!*"¹

¹ I have given an outline of the general bearings of the subject in a pamphlet published in Edinburgh in May, 1871, on 'The Physiology and Pathology of Mind in the Lower Animals.'

III.—Digitalis and Heart Disease.—By BALTHAZAR W. FOSTER, M.D., Professor of Medicine in Queen's College, and Physician to the General Hospital, Birmingham.

NEARLY one hundred years have elapsed since Dr. Withering published his celebrated "Account of the Foxglove." In that work the curative powers of digitalis in certain forms of dropsy, and its remarkable influence in diminishing the frequency of the heart's action, were proved by the investigations of a most able observer. A century's experience would appear to any one unacquainted with the slow evolution of scientific therapeutics, more than enough to have completed the inquiry begun by Withering, and to have determined the true position of the drug in relation to that organ, on which the first investigator noticed its effects. Not so. There are still to be found in the text-books of the profession the most diverse opinions as to its action, and the vaguest directions for its use. The drug which long ago earned the title of "the opium of the heart," is even now prescribed in the most opposite conditions, for the most contradictory reasons, and, as might be expected, with the most varied results.

The absence of clearly defined principles as to the forms of heart disease in which its good effects may or may not be expected, has led to all this confusion. Digitalis has been and is still ordered, when no exact diagnosis has been made; and this haphazard practice has necessarily led to widely different estimates of its value.

During the last few years my sphygmographic observations have led me to devote much time and thought to the study of heart disease, and the action of digitalis has consequently been to me a matter of daily observation and comment. It is impossible to go on using a remedy day after day in special forms of disease without drawing some conclusions as to its effects, and forming some rules for its administration. In this paper I propose to sketch the line of thought which has guided me in the use of the drug. The argument may, perhaps, possess little novelty, and the conclusions may not be new, but at all events I hope they may prove useful to others as they have to me.

In the beginning of my inquiry into the action of digitalis, finding no authority to guide me in the selection of cases for its use, I made a series of observations on the effects produced by it when administered to healthy persons, and to patients free from heart mischief. Giving it in moderate doses, one to three drachms of the infusion three times a day, I obtained results

agreeing with those of other observers. The pulse traces showed—

1. A diminution in the frequency of the heart's beats.
2. An increase in the force of each beat.
3. An increase in the arterial tension.

In cases of irregular action of the heart associated with cer-valvular lesions, there was observed what might be called a fourth effect, viz., the co-ordinating influence of the drug in restoring the regularity of the heart's movements. That these effects are produced by the action of digitalis through the sympathetic nerve on the heart and smaller blood-vessels, there is, I think, almost complete evidence. Whether the action is first on the heart or on the blood-vessels is another question, but one not primarily important to the present paper, in which I only propose to consider what indications we can gather as to the use of the drug from its admitted effects on the heart. In considering these effects, we must regard the heart mainly from a mechanical point of view; as a pump furnished with stop-valves to direct the stream of blood aright. When these stop-valves get out of order, there is no opportunity for repair, for there is no possibility of complete rest. The valve lesion, since it cannot be remedied by direct repair, is compensated for by alterations in the size of the heart cavities, and in the power of the heart muscle. By these means the defect of the valve is reduced to a minimum as regards its injurious action on the circulation. The pump still does its work, not perfectly, it is true, but often so nearly perfectly that with judicious aid it may perform its functions for years. It is the study of the mechanism of the compensation in each form of valve lesion which must be mastered, before correct ideas of treatment can be formed, or the true position of any drug as a remedy in heart cases can be defined. I propose to consider the several forms of valvular disease in succession, and I shall first take aortic insufficiency.

I. *Aortic insufficiency.*—In this form of valvular mischief we have a morbid change which acts only during the diastole of the ventricle. From the moment the contraction of the aorta or its contents takes place, and forces down the imperfect sigmoid valves, the blood pressure in the aorta drives a stream of blood between the insufficient valves into the dilating left ventricle. This regurgitation continues till the pressure of blood in the ventricle becomes equal to that in the aorta, that is to say, till the ventricle is filled ready for its next contraction. This period, during which the regurgitation acts, is very nearly half the period of each cardiac revolution. The results of this reflux are well known; the circulation is impeded, the balance between the contents of the arteries and the veins is disturbed by the loss which the arterial

side suffers during each diastole, and a state of low arterial tension is produced. If the insufficiency be suddenly created, a rapid development of pulmonary engorgement and a general venous stasis occur, which may end in death. In a case of this kind admitted under my care last year, in which the diagnosis of ruptured aortic valve was afterwards verified, the accident was followed by insensibility, general cyanosis, total absence of the radial pulse, &c., and it was only by the most untiring care in the administration of stimulants, and the judicious abstraction of some twenty ounces of blood, that Dr. Welch (then house physician) was enabled to restore the circulation. Aortic insufficiency acts on the circulation in a purely mechanical way. The aortic stop-valves are imperfect, and consequently the whole period of their action is a period of detriment to the efficiency of the circulation. To compensate for this, certain changes take place; the left ventricle dilates and hypertrophies, and the action of the heart becomes more rapid. More blood is thrown into the aorta, and less time is allowed for its regurgitation. What action would digitalis have in this case? It would augment the ill effects of the insufficiency by slowing the action of the heart. The diminution in the frequency of the heart's beats under digitalis, always means an increase of the period of the dilatation of the ventricles. Pulse traces readily show this. It is during this very period that aortic insufficiency produces its ill effects, and it is, consequently, not hard to understand why digitalis acts injuriously. The low arterial tension may require increasing, the heart's strength may want reinforcing, but these two indications cannot be fulfilled by any remedy which, like digitalis, adds to the prime evil. The more forcibly the heart beats, and the greater the arterial tension, the greater, *cæteris paribus*, will be the reflux through the imperfect valves; and while the valvular lesion remains a constant quantity, digitalis will multiply its effects by increasing the period of its action.

But digitalis has been known to do good in aortic insufficiency. I admit this. There is one condition which warrants its use—that of over-compensation. This is marked by violent action of the heart, bounding vibratory arteries visible all over the body, almost constant headache, flushed face, noises in the ear, occasional epistaxis, urine normal in quantity and free from albumen, &c. In these cases a few doses will do good. The action must, however, be watched, and the moment the pulse falls the remedy must be stopped; if not, syncope and other alarming symptoms occur.

A few weeks ago two cases in my wards served to illustrate these remarks to my class. The first was that of a finely-built

muscular man, æt. 35, with well-marked aortic insufficiency of some twelve months' standing. His general symptoms, as well as the characters of his pulse and impulse, indicated that the hypertrophy of the left ventricle was insufficient to render the compensation perfect. Three drachms of infusion of digitalis, given in half-drachm doses, were sufficient to aggravate all his troubles. The pulse became slower, but feebler in its tension, the dyspnœa increased, the urinary secretion was diminished, and a feeling of faintness and sinking was complained of, which gradually became almost intolerable. The continuance and repetition of the remedy increased these symptoms, the administration of brandy relieved them.

The second patient was a girl of 16, who had aortic insufficiency of some eighteen months' duration. In her case the very rapid incompressible pulse, the violent impulse, the flushed face, the almost constant frontal headache, the comparative absence of dyspnœa, and the free secretion of urine—all told of an hypertrophy of the left ventricle, sufficient, and more than sufficient, to compensate for the valvular lesion. Half-drachm and afterwards drachm doses of the infusion were given with the best effects. The headache was relieved, the pulse fell in frequency, the violent heart action was lessened; but even in this case the remedy could not be borne long. On the fourth day sickness and faintness and diminished flow of urine warned us to withdraw it. It is on this account that I generally prefer hydrocyanic acid, caffeine, and sometimes aconite, in these cases of over-compensation. They act equally well, and are less liable to cause unpleasant symptoms than digitalis.

II. *Mitral stenosis*.—Narrowing of the left auriculo-ventricular orifice by adhesion and contraction of the curtains of the mitral valve, constitutes a purely mechanical obstacle to the circulation, which acts, during the same period of each cardiac revolution, as aortic insufficiency. In this case, however, the filling of the ventricle is impeded by the narrowness of the passage through which the blood has to enter. While aortic insufficiency renders the ventricle overful, mitral stenosis keeps it too empty; yet both tend to drain the arterial side of the circulation: the former by allowing the blood to flow back, the latter by opposing its advance. Now, the period of ventricular dilatation being that during which the blood flows from the auricle to the ventricle, when there is marked narrowing of the channel of communication, the impediment can be compensated for in two ways. The propelling power must be augmented to drive the blood at a greater speed, or the time for its passage must be lengthened.

When the narrowing is not very great, and the demands on

the circulation by exercise are limited, a very perfect compensation is effected by the hypertrophy and dilatation of the left auricle and the right ventricle. By these means the current of blood through the lungs is made more rapid, the tension in the auricle is increased, and the velocity of the current filling the left ventricle is so multiplied, that the balance of the circulation is fairly maintained, in some cases for years. But when the narrowing is considerable, or some unusual strain has disturbed the unstable equilibrium on which the health of these patients depends, then the compensation which the auricle and ventricle offer fails. The rapid, irregular pulse tells of the varying quantities on which the ventricle contracts; sometimes the interval between the ventricular contraction is so short, and the ventricular charge is so small, that the systolic wave does not reach the wrist. Under these circumstances the second indication must be followed; the auricle must have more time to fill the ventricle. This, to say nothing of the increased power given to the cardiac muscle, is exactly what digitalis effects. By slowing the action of the heart, the period of time during which the blood from the distended auricle can flow into the ventricle is increased, and as the extra time allows more blood to pass through the narrowed mitral orifice before the final effort of the auricle is made, that effort is made on a smaller quantity of blood, and is, consequently, more effective; for the smaller the quantity of blood which the auricular muscle has to push before it, the greater will be the velocity given to the current. The ventricle, though contracting less frequently, contracts more effectually. Instead of eighty or ninety irregular contractions per minute, no two succeeding ones equal in force, and some so valueless that they are not perceptible at the wrist, we get some sixty steady equal beats. The pulse grows in force, fulness, and regularity; the arterial tension rises; the pulmonary congestion diminishes; the kidneys, before inactive, wake up to their work; and the advancing dropsy recognises its master, and beats a sullen retreat.

Yet even in this form of valvular disease, in which it is of all drugs the most valuable, digitalis has been declared to be inadmissible. It is, however, only in the very last stage of mitral stenosis that the remedy fails, and then because no drug can restore the functions of an organ irrecoverably worn out. I have more or less complete records of some twenty cases of this form of disease, and I think we may recognise three classes of cases. In the first the pulse is quite regular, or nearly so: the arterial tension, though low, is not associated with marked pulmonary congestion; and the patients, under favorable conditions, suffer but little. In these cases digitalis is not often wanted;

but whenever the patient complains of more than usual dyspnoea and diminished urine, with increased rapidity of pulse, it is a safe remedy, and one which almost invariably relieves the symptoms. To such cases among my out-patients I often give \mathfrak{zj} or \mathfrak{zij} of the infusion, combined with perchloride of iron, ter die. In the second class the pulse is irregular, and all the symptoms are worse. Then digitalis is still more striking in its effects, but to get these fully the patient should be kept in bed. In the last class it is generally too late to get good results. The albuminous urine, the pulmonary complications in the shape of oft-recurring hæmorrhage, and the general dropsy, tell that the heart is hopelessly unequal to its task. Occasionally, even in these cases, digitalis will succeed, especially if preceded by the abstraction of a few ounces of blood, so as to relieve the venous distension. The duration of the albuminuria is, in these cases, the most important point whereon to found an opinion; the chance of recovery is generally in inverse proportion to the duration of this sign. Two cases in which digitalis failed me unexpectedly, were examples of mitral stenosis, associated with a similar lesion on the right side of the heart. In the first case the digitalis was powerless all through; in the second it saved life several times, but on a last occasion failed, and then, after death, as had been diagnosed during life, recent tricuspid narrowing was found.

III. *Mitral Insufficiency*, like aortic obstruction, differs from the two preceding forms of valvular disease, in the time during which the valve defect comes into play. It is a deficiency of stop-valve action, which occurs only during the systolic period of each cardiac revolution. The valve action, too, differs from that of the sigmoid valves, inasmuch as it does not depend solely on physical conditions, but is the result of muscular energy. The small muscles which are attached by such a wonderful arrangement of their tendons at the edges of the two curtains of the mitral valve, must act normally to ensure the perfect function of the valve. But the muscular action does not end here. Recent observations¹ go far to prove that the closure of the auriculo-ventricular orifices is mainly, if not wholly, a muscular act performed by the contraction of the fibres which surround each orifice. The stronger the contraction of the ventricular muscle therefore, the more perfectly will the orifice be closed, and regurgitation checked. The curtains of the valve in health so overlap that between them no passage is left, and the approximation of the muscular sides of the orifices prevents the thin valve from having to bear the whole pressure of the blood during the

¹ Onimus. 'Journal de l'Anatomie et de la Physiologie, Juillet, 1865. Paris.

ventricle's contraction. In disease, the valves no longer meet in such perfect apposition, and in the space between them the blood finds a regurgitant passage. The compensation by which this is met consists in dilatation and hypertrophy of the left auricle and right ventricle, aided by hypertrophy and dilatation of the left ventricle. The last cavity by the form of its increase contains its extra quantity of blood in the upper part of the ventricle, nearer to the aorta, and more out of danger, as it is further removed from the unapposed edges of the mitral valve. There is less blood at the apex of the distended cavity where it can regurgitate, more at the base where it cannot. To keep up the compensation there must, however, be good steady muscular action, no wavering in the contraction, no inefficiency in the sphincter-like narrowing of the auriculo-ventricular orifice, and no unsteadiness in the action of the papillary muscles. Any extra effort soon disturbs the artificial equilibrium, and confused muscular action follows. It is this which digitalis corrects. In place of a large number of ineffective contractions, it concentrates the power of the ventricle on a smaller number of well-directed, steady beats, each throwing a larger charge of blood into the arteries, and so diminishing beat by beat the over-distension of the right heart. The right ventricle so aided, is also aiding, by the more vigorous efforts which the digitalis enables it to make. In this form of disease all turns on the healthiness of the cardiac muscle; the remedy will do no good, nay, rather will do much harm, if muscular degeneration has occurred. In the cases in which it has disappointed me I have invariably found after death the heart cavities dilated and the muscles altered.

IV. *Aortic Obstruction.* In this condition, the ventricle having to drive its contents through a narrowed outlet, must have either more time to do it in or greater propelling power. The compensatory dilatation and hypertrophy ensure both. The ventricle contracts on a larger quantity of blood than is normal, and takes longer to drive it into the aorta. The systolic period of each cardiac revolution is lengthened, and the diastolic period is proportionally increased. In this case we have at once the simplest and the most perfect of any of the forms of compensation. Digitalis can do little for such cases, because there is little good to do. As long as the cardiac muscle is well nourished there is no need for the remedy; when the muscle ceases to be healthy digitalis is worse than useless. The heart acts in these cases slowly and forcibly, and the slowing action of digitalis becomes its dangerous action, and more than counterbalances any good effects which might be expected from its action on the cardiac muscle.

In a case of pure aortic obstruction I ordered digitalis. The pulse fell under its use in the course of three days from 62 beats per minute to 50. This diminution in the frequency of the heart's action was associated with no improvement in the symptoms. On the contrary, the patient, who had been comfortable before its occurrence, now complained of a feeling of faintness and giddiness, and had decidedly increased dyspnoea. The remedy was withdrawn, and all these symptoms disappeared. Some few years ago, a man came under my care for chronic bronchitis, whose pulse beat ordinarily only 27 times a minute. He had no discoverable heart disease, and some years after the heart was found to be healthy. Digitalis was cautiously administered to this patient; the pulse fell to 23 and 24 per minute, and a sense of precordial anxiety and slight attacks of vertigo were produced. On the discontinuance of the drug these symptoms ceased to trouble him, and the pulse returned to its ordinary rate of 27.

It is only when the hypertrophy exceeds the limits of compensation that digitalis is useful; then, by slowing the pulse and regulating the heart muscle, its good effects are well seen.

The remark may be made that, in practice, we do not meet with the simple valvular lesions I have discussed. I readily admit this truth. Valvular diseases are, as a rule, complex. Aortic insufficiency is often (though not so often, I think, as the systolic murmur would lead us to infer) associated with obstruction; the two forms of mitral disease are frequently conjoined; or mitral in either of its forms, or double, may coexist with double aortic murmur. But for all these cases the considerations I have advanced are, in my experience, sufficient to guide our practice aright. Both forms of aortic disease are present; we have to treat the one which predominates, and to which the general symptoms are due. This is almost always the regurgitation, and digitalis is hurtful as a rule. In double mitral we have a coincidence of two lesions, both demanding digitalis, and we may give it with the best effects. When mitral regurgitation is associated with aortic we have a rupture of the compensation of the aortic lesion, and digitalis will not help us to remedy it. The rule is, in all cases, to treat the predominant valve lesion, and never to expect good from the use of digitalis when the heart muscle is unsound. Guided by the principles I have sketched, it may be given most usefully and most safely. I constantly prescribe it for hospital out-patients from a distance, and continue its use for weeks. I nearly always prescribe the infusion, and test its continued action by the effect on the secretion of urine. As long as the quantity of urine increases or keeps up to the maximum, which the digitalis has produced, the drug

is acting beneficially. Not long ago, one of our most eminent authorities on heart disease wrote:—"In cases of mitral valvular disease I believe, however, that digitalis is eminently useful, not by any influence which it exerts over the heart itself, but from its powerful diuretic action."¹ This sentence is full of practical wisdom; but we may now say, I think, that the beneficial effects are not, as Peacock put it, on account of its diuretic action, but the converse; the diuretic effect is the result, the outward and visible sign of its beneficial action. The diuresis indicates a restoration of the normal balance between the contents of the arteries and the veins, an increased arterial tension, and, consequently, a refilling, under normal pressure, of the empty capillaries of the Malpighian bodies. The high-coloured scanty urine, loaded with urates, is replaced by a clear and copious stream, which tells of a steadily beating heart and a firmer pulse.

¹ 'On some of the Causes and Effects of Valvular Disease of the Heart.' By Thos. B. Peacock, M.D., F.R.C.P.

Chronicle of Medical Science.

REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIÈNE.

By BENJAMIN W. RICHARDSON, M.D., F.R.S.

I.—TOXICOLOGY.

Antagonism of Poisons.—Dr. John Reece, Professor of Medical Jurisprudence in the University of Pennsylvania, has conducted a series of experimental observations having reference to the supposed antagonistic action of morphia and atropia, morphia and prussic acid, morphia and strychnia, morphia and aconitia, morphia and arsenic, strychnia and tobacco, strychnia and aconitia, strychnia and tincture of chloride of iron, strychnia and tincture of iodine, strychnia and Calabar bean, and atropia and Calabar bean. He communicates now the results he has obtained by experimenting with opium and hydrocyanic acid. The animals submitted to experiment were dogs, and as a preliminary study the toxic action of both agents were separately tried on dogs. It was found that both morphia and hydrocyanic acid would induce certain well-marked specific effects. Next, morphia and hydrocyanic acid were given in combination. To a strong full-grown dog two grains of morphia and one fluid drachm of ordinary prussic acid were administered in three divided doses at intervals; the animal ultimately recovered. The symptoms observed and recorded evinced a decided preponderance of the effects of the morphia over those of the prussic acid, which latter, however, was in a minimum quantity. In some further experiments the two poisons were given in a more equal dose, each in full dose, when the symptoms of each toxic agent were clearly manifested. It was further proved that morphia does not counteract the fatal effects of prussic acid unless it be in large excess over the latter, and not then if the acid be taken in a full poisonous dose. In another series of experiments it was shown that in cases of death from the combined poisons the death is undoubtedly attributable to the hydrocyanic acid rather than to the morphia. It was observed by the experimentalist that in one case, where hydrocyanic acid alone was used, recovery was greatly aided by cold affusion. The inference drawn by Dr. Reece from all his experiments is, that the antagonism between prussic acid and morphia is very slight, if, indeed, it exist at all.

In a recent criminal case, the case of Dr. Schœppe, which has

recently excited considerable interest in America, it was strongly affirmed by the prosecution, says Dr. Reece, that the deceased had died from having taken a combined dose of morphia and prussic acid, although the chemical evidence failed to establish the presence of either of these poisons after death, and the symptoms before death were utterly inconsistent with the existence of at least *one* of the alleged poisons, namely, prussic acid. The testimony most relied on by the prosecution, to sustain this very extraordinary allegation, was that of a physician who undertook to say that the symptoms of the deceased, which were really only the ordinary symptoms of apoplexy, were such as would result from the combination of morphia and prussic acid, because some thirty years previously he (the physician) had administered to a chicken hawk a mixture of corrosive sublimate, opium, and prussic acid, and the bird died the next day, after exhibiting certain symptoms, which in his opinion resembled those of the deceased.—*American Journal of the Medical Sciences*, January, 1871.

Action of Atropia on Birds.—We have noticed in previous reports the excellent observation of Dr. Weir Mitchell, that morphia, except when administered hypodermically in very large doses, has no fatal poisonous effect on pigeons. Dr. Wood, Professor of Botany in the University of Pennsylvania, has now extended some observations to other alkaloids, and has tested the action of atropia on pigeons. In a first series of experiments he tried to determine the local action of belladonna upon the pupil of the pigeon, and showed very distinctly that the alkaloid, even when used in strong solution, three grains to the drachm, is inactive. In another series of experiments he attempted to learn whether or not belladonna given internally could act on the eye, and he confirms the statement made already on this point by Mr. Wharton Jones and Mr. Lemattre, that it will not. In one instance there was decided contraction of the pupil.

A third series of experiments had reference to the dose of the alkaloid that could be tolerated by pigeons, and the result showed that belladonna, stramonium, and hyoseyamus, resemble morphia in their want of action on these birds. It appears to be almost impossible to kill them with the extracts given by the mouth. Doses of over fifteen grains are generally vomited, and in one experiment forty-eight grains of the best English extract were given in a little over seven hours without any appreciable effect. In another instance three grains of a beautiful and pure sulphate of atropia were given and retained, and yet did not appear to interfere materially with the well-being of the pigeon.

Injected hypodermically, atropia will kill a pigeon, but not less than two grains are required, a dose equivalent in toxic effect to ten grains of morphia on the human subject. It was supposed by Wharton Jones that atropine would not dilate the pupil of birds, because their pupils contain no radiating fibres. Dr. Wood disputes this explanation; and in respect generally to the causes of the inactivity of

atropia on pigeons, he holds these to be three in number. Firstly and chiefly, absolute obtuseness of the nerve centres to the action of the agent, the cause of which we shall probably be able to explain just so soon as we know why atropia acts on the nerve centres on man. Secondly, very rapid elimination of the poison, the conjoint urino-fæcal discharge being watery and abundant under the action of the drug. Thirdly, when taken internally, a comparatively slow absorption, so that the poison is removed from the blood by the kidneys as fast as it enters it.—*Ibid.*

Bromide of Potassium as an Antidote for Strychnia.—Dr. W. W. Hewlett, of Babylon, relates the case of a farmer who took, by mistake, five grains of strychnine, procured for the purpose of destroying rats, on the evening of November 14th, 1870. Immediately after taking the strychnine he is reported as having retired to rest, and as sleeping for two hours, when he awoke, “feeling much confused, and as if something was going to happen.” Pains in the abdomen and twitching of the limbs soon followed, with violent tetanic convulsions and opisthotonos. The patient was of very intemperate habits, and his friends, thinking the symptoms due to alcohol, gave him an elixir of opium, which he had taken before for the purpose of quieting the muscular tremor and restlessness of alcoholism. The remedy giving relief, it was administered as he called for it, and he took six teaspoonfuls in the course of two hours. Nausea and vomiting soon ensued, when he felt better, and remained quiet for two or three hours. The pain and spasms coming on again, he drank freely of water, in order to produce the vomiting which afforded so much relief before. He continued in this way until 5 a.m. of the 15th, imbibing freely of cold water and vomiting profusely: violent tetanic convulsions occurred at intervals; at 5 a.m. the paper which had contained the strychnine was found, and Dr. Hewlett was sent for immediately.

When Dr. Hewlett arrived he found the patient unable to move the extremities, but the intellect was clear. His head was drawn forcibly backwards, and he was suffering the most painful and violent spasms, provoked by the slightest attempt to move, by the entrance of any person in the room, or the closing of a door. As the man could swallow, Dr. Hewlett gave him ninety grains of bromide of potassium, “*a dernière ressource*,” every half hour. In twenty minutes after the administration of the first dose there was perceptible improvement, which continued. In two hours the man could move his arms. The bromide was then given at the rate of a drachm every hour; but the convulsions coming on again with greater severity, the remedy was given for one hour at intervals of fifteen minutes. At the end of the hour the patient felt easier again, and the bromide was continued, in smaller doses, at intervals of half an hour to two hours, according to circumstances, during the day and the following night. In twenty-six hours from the time when the bromide was first given he was walking about, feeling a little weak, and occasionally with a slight twitch. The case ended in recovery.—*New York Medical Journal*, March, 1871.

[This case is one of singular interest. It is clear that the first steps of the recovery were brought about by the vomiting, and it is possible that the recovery might have occurred even had no bromide been administered. But it is also clear that the bromide of potassium counteracted the tetanic and convulsive action, and in that way sustained the sufferer, and promoted and expedited recovery. It would be a useful research to ascertain whether the bromide of potassium has any immediate controlling influence over strychnine, and if so, the doses in which it proves to be antidotal. We have ourselves, in experimenting with bromide of strychnine, inferred that the action of the alkaloid is modified by the bromine, the intensity of the action being reduced, the period of action prolonged.]

Mode of Action of Arsenic in Large and in Small continued Doses.—Of late months the scientific labours of our distinguished confrères in France have, by dire necessity, lapsed into abeyance, to the great loss, especially, of the department of scientific progress chronicled in these columns. We welcome, then, with peculiar pleasure the reappearance of this long-lost labour in a paper by M. Blachez, on the action of arsenic in large and small doses. A large dose of arsenic, determining a rapid poisoning, produces, says the author, a violent phlegmasia, symptoms of a choleraic type in the digestive canal, a burning heat of the alimentary tubes, suppression of urine, cramps, and a progressive coldness of the body; syncope, convulsions, often localised paralyses specially attacking the extensors, as in saturnine paralysis, delirium, and coma. The symptoms show the intense attack to which the nervous system has been subjected. The pulse is usually accelerated, irregular, small, and compressed; but sometimes it is strong and full. This contradictory effect depends upon the period at which the observation is made.

The falling of the temperature of the body is a fact generally observed.

The study of the physiological action of arsenic, administered internally and acting by absorption, is surrounded by difficulties, and the interpretation of its effects occupies the best thinkers of the day. Who is unaware of the facts pointed out by Schallgruber, Flechner, and, above all, Tschudi (1822-1851), respecting the arsenic-eaters of Styria and Austria? All these facts have been confirmed by the best authorities, and are now beyond a doubt. The ends above all most desired by these arsenicophages is to obtain a freshness of face and a certain degree of embonpoint, also a facility of respiration in climbing ascents; they feel much lighter and gasp much less. The doses vary from two to three centigrammes. From constant use they take twenty or even twenty-five centigrammes. In Austria the horse-dealers administer arsenic to the animals they wish to sell. The horses thus treated look better nourished, and yield more profit. Their skin is glossy, and their forms rounded. The coachmen of Vienna have the same habit for improving the appearance of their horses. These facts present a certain analogy with some others pointed out by the author. He first explains the facts he has

observed, and then the different interpretations which they have received. When we give arsenic in small doses we observe effects more or less constant; that is to say, it is not uncommon to observe some who can endure arsenical treatment without any visible reaction. When a little heat is felt in the pharynx and in the course of the œsophagus after arsenic, the appetite usually improves, digestion is more rapid. When the burning continues, when there is nausea, and diarrhœa with colic, it must be presumed that the useful dose is carried too far. As an immediate result of increase of appetite nutrition proceeds better, and embonpoint increases. The action of arsenic on the respiratory functions in the normal state is not clinically demonstrated; but it is incontestable that in certain diseases of the respiratory organs, arsenic produces beneficial results.

With respect to the circulation, there is often observed a rosy colour on the face. A therapeutic dose of arsenic does not appear to have any well-defined action on the heart; but the same dose seems to have the property of relaxing the skin, while a poisonous dose accelerates action. One of the most constant results of arsenical treatment is the very marked diminution in the quantity of urea eliminated in twenty-four hours.

The temperature diminishes evenly. The central nervous system does not appear to be affected by medicated doses. The urinary secretion is increased. One sometimes observes a little salivation, but more often diarrhœa. The skin is not constantly affected by the prolonged use of arsenic in small doses. However, it produces, in certain cases, a modification of the pigmentary matter, demonstrated by brown marks on the skin, which continue a long time. In addition, certain forms of eruptions which progress almost to pustular eruption, with swelling of the eyelids and watering of the eyes, sometimes occur.

The modes of elimination are multitudinous. Arsenic eliminates itself by the skin and the divers secretions of which it is the seat. M. Chatin has found it in the serosity of a blister, MM. Begeron and Lemaître in the sweat. Besides, its presence is in some measure furnished by the eruptions observed in certain cases.

Arsenic may be found in the saliva and in the tears; the bilious secretions may contain considerable quantities of it, and that which proves the importance of the liver as a means of elimination is the fatty change of this organ in cases of arsenical intoxication. It is, however, by the kidney that arsenic specially is eliminated. When the elimination is in abundance the secretion contains a certain amount of albumen.—*Gazette hebdomadaire de Médecine et de Chirurgie*, 1871.

On the Katipo or Poisonous Spider of New Zealand.—Mr. F. W. Wright, of Toronto, records that, in the month of December, 1868, a person of the name of John Huff, living near his residence, came into the surgery complaining that he had been bitten by a spider on the shoulder. He was in the employment of Messrs. Orchard and Brown, of Stanley Street, Mechanic's Bay. He was occupied at

the time in carrying firewood to supply the furnaces of a brick kiln. The wood was stacked near the kiln in sedge or coarse grass. This happened between the hours of 11 and 12 a.m. At noon he came home to dinner, sat down to table, but, upon attempting to eat, found he could not open his mouth, or was scarcely able to articulate, in consequence of stiffness about the jaws. He was alarmed and came into the surgery, when it was difficult to understand what he had to say. All that could be learned from him was that he had been bitten by a spider on the shoulder in the bay. Upon examining the spot Mr. Wright found the surface raised to an extent as large round as a teacup. This elevated spot was white, and it was surrounded by a halo of red, not unlike an exaggerated wheal of the nettlerash. He complained of considerable pain of the part, and during the examination became faint, and soon almost pulseless. His pulse was unusually slow, counting scarcely more than twelve or thirteen beats in the minute. His countenance and the general surface of the body assumed a hue of extreme pallor, which gradually turned to a blue tint. His extremities were cold and flaccid, his respiration almost ceased, and he appeared about to expire. Dr. Pinching was in the house at the time, and was called to render assistance. He was astonished at the feebleness and prostration of the patient from such an apparently trifling cause.

From his extreme faintness it was necessary to lay him on the floor, when Dr. Wright applied spirits of ammonia to the wound, which had the effect of lessening the swelling and abating pain. He also administered ammonia and water, afterwards combined with brandy, in considerable doses. Under this treatment his pulse gradually improved; his circulation and respiration became more natural, as was evinced by his return to a more natural colour. Although a stout, strong man, this state of depression lasted for upwards of two hours before he was able to return home. In the evening Dr. Wright found him considerably improved, having taken a small dose of medicine. For several days he could not return to his work, but complained of great lassitude and nervous depression, which he was sensible of for many days after.

It must be evident from the symptoms of this case that the man was powerfully affected by a narcotic and irritating poison, which, being absorbed into the circulation, affected the heart, brain, and nervous system, to a very considerable extent, almost amounting to fatal syncope; and that the stimulants, by exciting the heart's action, gradually aroused the excretory functions, so as ultimately to remove the poison from the system. Although suffering under the influence of the poison for a considerable time, it does not appear to have left any permanent effects behind it, for the man has since been in perfect health.

In corroboration of the nature of this accident the following graphic description of the bite of the Katipo is appended by the Rev. Mr. Chapman, whose long residence as a missionary to the Maori race, in the interior of New Zealand, renders his observations and opinions of peculiar importance. In the course of his sojourn in

New Zealand he had three rather remarkable proofs of the violently poisonous nature of the bite of the Katipo. Some twenty years ago a party of natives had taken up a temporary residence at Waihi, near Maketu, their resting place being near the sea-beach. During their morning's meal a girl was bitten by a Katipo in the region of the abdomen. She did not seem at first to suffer much pain, but towards noon inflammation set in, and some native remedies were used. As these had no effect her friends decided to convey her to Mr. Chapman's residence, and they reached his house about 1 p.m. Mr. Chapman discovered, on first seeing her, indications of great pain, and on examining the wound found a swelling of the size and shape of the obtuse end of a hen's egg. He immediately rubbed the part with strong ammonia. This had no other effect than of lessening the severity of the pain, it failed in causing decrease of the swelling. He also gave the girl medicine, probably salts and tincture of henbane. After this he saw her nearly every day for a fortnight, using such means as appeared suitable to him. She seemed at this stage to be gradually recovering, but suddenly became faint and pallid, lost all desire for food, and although offered whatever the house afforded, would only take a little bread and tea, and sometimes a little wine. She lingered in this way for about six weeks and then died.

The next case was the son of a tailor residing at Maketu. Three of his boys went up the river on a ramble, and lingered at the Tumu, resting themselves by sitting on the tufts of sedge growing on the sandhills just above the reach of the tide. These tufts are the principal haunts of the Katipo. While so resting one of the boys was bitten on the fleshy part of the thigh by the insect having, unperceived, crawled up his trousers. The boys were at this time about two miles from home. They returned immediately, but, not thinking the bite of any consequence, delayed applying to Mr. Chapman until towards the evening, at which time the sufferer became ill and the place bitten inflamed. Mr. Chapman attended him, using the same remedies as in the other case; but the lad soon showed signs of wasting and loss of all energy, with the appearance of being in a decline. He was about three months before he rallied, and about another three months before he fully recovered.

The next case occurred to Toke, the chief of Maketu. He was travelling up the coast from Whakatane, and, halting to dine, he seated himself on a large tuft of sedge. He had hardly been resting five minutes before he sprang up and said, "I have been badly bitten by a Katipo." He was bitten on the upper part of the thigh. The Rev. Mr. Chapman, who was of the party, directed him to lie down; he then dissolved some carbonate of soda in a very small quantity of water, and added to this some brandy from his flask; he quickly made a crucial incision over the part bitten, and forcibly squeezed out the blood and rubbed in this antacid solution, keeping up this action alternately for some ten minutes, when Toke said he no longer felt the pain. He remarked, on rising, to Mr. Chapman, "Had you not been with me, I should have

had a long illness." Only two or three minutes could have elapsed after the bite before a spot about the size of the top of the little finger appeared, and of a peculiar white colour, in strong contrast with the dusky shade of Toke's skin. Toke was careful to secure all the blood that had been forced out of the wound, by absorbing it in a piece of rag torn from his shirt; and the relic, now doubly sacred, he carried into the middle of a swamp close by, and was seen stamping it down violently into the ground to preserve it from possible desecration.

The natives generally avoid sleeping on the sea-beach, and have no fear of the Katipo half a stone's throw inland of the sea-beach line. They never (of themselves) use any other remedy than rubbing and applying hot half-scalded leaves to the part, and as soon as convenient taking the patient to the priest to receive the benefit of his incantations; they believed in the efficacy of prayers made to their gods of the hills and valleys.

Here, then, are the evidences of a narcotic and irritating poison, the absorption of which into the system produced more permanent effects upon the body, causing a degraded condition of the strength and tone of the constitution, and in all probability a blood-poisoning that led to the subsequent disease. In all these cases the effect of the poison may in some degree have been modified by the condition of the insect's poison-bags, and by the locality and character of the bite. Under any circumstances, however, it is plain that the deleterious effects of the bite of the Katipo, and its poisonous character, have long been recognised and feared by the natives. In Toke's case one can but admire the skill and decision of the missionary, who, all alone in a wild and savage land, could have treated the case so actively and with such good effect; he, in all probability, prevented the absorption of the poison into the system by the means he employed.

From all the accounts that can be collected, the Katipo is a small spider of about half an inch to three quarters of an inch in diameter, measuring across the body and legs, according to the authority of Major Heaphy, who, having been Surveyor-General of the Colony, has had abundant opportunity to know the insect, and is familiar with its resorts.

The Katipos are said to be of two kinds: one has a dark glossy body, with a marked red spot on the back; the other, of about the same size, has a similar round black and shining body, but is without the spot.—*Transactions and Proceedings of the New Zealand Institute*. Edited by Dr. Hector, F.R.S. Vol. ii, 1869.

On the Poisonous Principle of the Tutu Plant (Coriaria ruscifolia).—Mr. Skey, the analyst to the Geological Survey of New Zealand, has made a series of researches on the active principle of the poisonous plant known as the "Tutu." He operated on specimens of the seeds of the plant furnished by Mr. H. H. Travers. The plan adopted was to separate as well as he could all the more immediate proximate constituents of the seed (in which the poison is

known to exist), and to test each likely one by itself, as to its effects upon the animal economy.

First, he extracted a portion of the finely ground seed with cold water, and another portion with weakly acidified water, and treated them separately by a new process, now much in vogue for the separation of alkaloids (process of Rogers and Girdwood), all the evaporations being conducted at a temperature not exceeding 90° Fahr.

The residuum from these processes was very small, and gave no indications of the presence of alkaloids to the proper tests; it consisted almost wholly of gum.

The result seemed to dispose of all that was soluble in water or weak acids, and, to a certain extent, impugned the correctness of the general idea that this poison is of the nature of an alkaloid.

The part of the seed insoluble in the reagents was next examined.

Alcohol was passed through this repeatedly, and the extract evaporated, when a large quantity of a greenish-red coloured substance discovered itself; this, treated with ether, separated into two parts, one a green-coloured oil, soluble, the other a resinous substance, quite insoluble in this menstruum.

The resinoid substance was reserved for after-examination, and the oil at once tested in regard to its effects on the animal economy. For this purpose Mr. Travers administered about five minims of it to a full-grown cat, after a twelve hours' fast. The oil acted as an emetic in a short time, and a great portion of it was vomited. In half an hour, however, the animal showed signs of uneasiness, with convulsive twitches of the ears and eyes, together with a forward jerking of the head; also much frothing of the mouth, culminating in a convulsive fit, in about one hour after the dose was administered. After a little while this fit passed off, but the twitches and forward jerkings continued; a second very severe fit, of short duration, occurred in about one hour afterwards, after which the cat gradually rallied. These symptoms agreed generally with those exhibited by cattle and sheep when poisoned by this plant.

Although Mr. Travers made but one experiment, it must be allowed that the result has fairly proved that the poison of the seed—and, by a very proper inference, the poison of the plant generally, since he finds an oily substance throughout it—exists in this oil, if, indeed, it is not the oil itself. It therefore now only remains to be ascertained whether this oil is a single proximate substance or a mixture or compound of such; and if the latter, which is or which are the active substances concerned in the production of the phenomena he has described. Unfortunately, he had not sufficient oil to allow him to test this properly, but he will have large quantities of the seeds next autumn from Taranaki.

The following are the characteristics of the oil, as ascertained up to the present time:

It is somewhat viscid at common temperature, but flows freely at a little above this; colour, pale green; reaction, acid; taste, bland; it burns away readily, with much flame; is scarcely volatile without

decomposition; is soluble in ether, alcohol, chloroform, and strong acetic acid; is insoluble in hydrochloric or nitric acid, and is also insoluble in water; it does not dry when long exposed to the air.

When boiled with solutions of the caustic alkalies there is much frothing, but only a portion of the oil dissolves, even when the boiling is continued for many hours; the portion dissolved is found to be saponified. The whole of the oil is, however, soluble in a cold alcoholic solution of potash, without yielding a precipitate when admixed with water; hence, it is probable that all the acid portion of the oil is really saponifiable, that which was unsaponifiable in the first instance being a product of the metamorphosis of a portion of the normal oil by the process employed.

When the oil is heated to the decomposing point a substance is given off having the pungent odour of acrolein, a substance characteristic of the presence of glycerine or oxide of lipyle, the base of common fatty bodies.

Heated with caustic alkalies, either in the wet or dry way, there are no alkaline vapours evolved, but in the latter case an odorous oil forms, probable *œnanthylie* acid.

From the reaction of this oil, here described, it evidently belongs to the series of non-drying fixed oils; in its solubility in alcohol or acetic acid it bears a remarkable resemblance to castor oil, the only other fixed oil which Mr. Travers asserts to be wholly soluble in acetic acid. Castor oil, it will be remembered, is a very peculiar oil; it does not contain any of the acids of the common oils or fats, but, in place of them, two very singular acids, quite peculiar to this variety of oil. Hence, Mr. Travers conceives the acid part of this oil of Tutu to be also quite distinct from the ordinary fatty acids; to be, in all probability, peculiar to it; and to one or more of these acids he ascribes the poisonous effects of this oil.

If further experiments should confirm the correctness of the views here stated, this case will, says Mr. Travers, become invested with an interest beyond that immediately under our notice, since it will offer another instance in which a non-nitrogenous oily principle is proved to affect the system like a neurotic poison, this class of poisons being almost always alkaloids, or at least nitrogenous substances.

It will be remembered there are several poisonous plants in Europe which have hitherto refused to yield any pure poisonous principle to chemical processes; but then these processes have been, he says, as a general rule, especially for the detection of alkaloids. With this case to point, therefore, it seems in the highest degree probable that in some of these cases, at least, the poisonous effects may be due to a non-nitrogenous oil, not yet isolated or examined. In view of this Mr. Travers has recommended the subject for examination to a friend of his residing in England, and he expects in a few months to hear something more of this, or else to have selections of seeds, &c., from the plants he has named in his letter, so that he can inquire into the subject for himself.

With regard to antidotes for administration to animals, &c., poi-

soned with the Tutu plant, Mr. Travers is inclined to think that, in addition to emetics and purgatives, very dilute acids would be beneficial, since, by preventing saponification of the oil, they would tend to keep it insoluble, and therefore inert. Since this paper was read Mr. Travers has learned from the 'London Chemical News' of August 6th, 1869, that M. Van Ankum has discovered the poisonous principle of the *Cicuta virosa* to be an essential oil, of formula $C_{10}H_8$, but "could not find any alkaloid in this plant at all." This was one of the plants especially selected for examination in the communication alluded to above.—*Ibid.*

IV.—SUMMARY.

De la Contagion Morale. Par Prosper Despine, M.D. Marseille, 1870.—This work, for it is a small separate volume of twenty-four pages, is full of interest for all classes of men; but specially for the jurist, medical jurist, physician, and politician. The author writes as a scholar, and, at the same time, as a faithful and original interpreter of natural phenomena. There is, he explains, moral as distinctly as there are physical contagions.

De L'imitation considérée au Point de vue des différents Principes qui la déterminent. Par Prosper Despine, M.D. Marseille, 1871.—This work, also distinct and by the same author, bears on a subject somewhat similar to that named in the work immediately above. The author here treats of what he calls "La contagion nerveuse, contagion organique." There is, he says, an instinctive imitative faculty, but there is also an acquired faculty, caught, so to speak, as a physical ailment may be. The book is admirable.

On the Immunity from the Action of Morbific Agents. By D. Francis Condie, M.D., 'American Journal of the Medical Sciences,' January, 1871.—Dr. Condie believes in the immunity of some persons from the effects of certain poisons which in other persons act with energy. He applies his reasoning specially to vaccination, and considers that of a hundred persons vaccinated, apparently with complete success, there will always be found some, probably about sixty-seven per cent., who will be endowed with complete and permanent protection from smallpox. The remainder will not be permanently protected.

Signs of Drowning. By James Forrest, M.D., C.M.—This work by Dr. Forrest is published in a separate volume of seventy-four pages. The author deals fairly and clearly with his subject, and supplies a series of cases, in illustration of death by drowning, of great value. The cases, fifty in number, are tabulated so as to show signs of apnœa, signs of drowning, and condition of the body. Dr. Forrest shows that hyperæria (balloon lung of Casper) and the apnœal signs are the only signs of drowning obliterated by putrefaction. The blood after drowning is easily decomposed, and is the chief source of the gases which render the body buoyant.

The great Equatorial Current, misnamed the Gulf Stream. By Vice-Admiral Sir Edward Belcher, K.C.B.—Sir Edward in this very able paper reaffirms and restates the views he has maintained for so many years against the commonly received idea that the so-called gulf stream is the cause of the sustained temperature of these islands, and that sudden variations in the temperature of our climate result either from changes in the course of warm streams, which are centrifugal from the well-known equatorial current of the Atlantic, or from icebergs driven southwards by gales from the arctic regions. The paper is startling, but not on that account less excellent.

Spécifiques pour le Pansement des Plaies et Blessures soit à l'état de Charpie, soit à l'état de Compresse. Par M. Donato Tommasi, Sci.D.—In this essay, of a few pages, the author gives the formulas for several preparations, hygienic and curative. There is, however, nothing that is new to the English medical reader.

Medico-Legal value of Confession as an Evidence of Guilt. By Dr. W. A. Hammond, 'The Medical Record' for February 15th, 1871.—Dr. Hammond in this paper argues, with his usual force, against the too ready acceptance of confession as proof of guilt, and he suggests the possibility that the confession of Constance Kent of the murder of her brother may after all be a delusion. Confession, he maintains, is no evidence of guilt, and should not be admitted unless confirmed by collateral evidence.

Physical Culture in Amherst College. By Nathan Allen, M.D. (Separate treatise.)—The work of Dr. Allen, prepared by order of the trustees of the Amherst College, describes the practical advantages of the lighter gymnastic exercises for boys and youths, and is a good common-sense essay. Health and strength, says Dr. Allen, are not synonymous terms. A person may have great strength in his limbs or in certain muscles of the body, but really not have good health. It is in the exercise of all the muscles of the body that gymnastics prove useful.

Report of the Association of Certifying Medical Officers of Great Britain and Ireland.—The third annual report of the certifying medical officers contains a very able address by Dr. Arlidge on the first report of the Royal Sanitary Commission, and on the two last half-yearly reports of the Inspector of Factories. Dr. Arlidge is of opinion that the work of the certifying surgeons should much rather be extended than abridged, and he urges that the judgment of the medical man should not only be called upon for deciding on general fitness for work, but for determining on fitness for the particular work to be entered upon.

[We regret that from the pressure on the space of the Review this quarter the publication of the parts of our report relating to Insanity, Hygienic and Forensic Medicine, together with remainder of the Summary, is unavoidably postponed.]

REPORT ON SURGERY.

SELECTIONS FROM THE FOREIGN JOURNALS.

BY ALFRED POLAND, F.R.C.S.

Des Abscès Perinephriques. Par M. TROUSSEAU. 'Clinique Médicale,' 2nd edit., tom. iii, p. 718. Paris, 1865.

On Perinephritic Abscess, its complication and its treatment, with Cases by Dr. BOWDITCH. 'Medical and Surgical Reports of the Boston City Hospital,' vol. i, 1870.

THIS special disease has only of late years engaged the attention of the profession, and it is to M. Trousseau that we are indebted for an able and elaborate exposition of its nature. He describes its insidious onset, its slow progress to the formation of an abscess, its etiology, its symptoms both local and general, its termination and treatment, all of which are based upon many cases narrated in the course of his interesting article. Dr. Bowditch has brought together ten cases which have come under his notice, and he offers remarks upon the complications and the mode of treatment to be adopted. The reviewer of his article has, in the 'American Journal of Medical Sciences' for April, 1870, fully described the symptoms and diagnosis mainly derived from Trousseau. Dr. Duffin has published in the 'Medical Times and Gazette for Sept. 24, 1870,' an interesting case of perinephritic abscess successfully treated by incisions in the loin; it was, however, one of consecutive abscess, following disease of the kidney, and the cases and examples quoted by him chiefly refer to this form of disease. Trousseau has carefully avoided the latter class of cases, although he alludes to one or two examples;—he confines himself rather to primitive and purely local perinephritic abscess without any other lesion. He distinctly points out, in the study of the symptoms, two important divisions:—1st. Those cases in which the perinephritis is independent of renal lesion; an inflammation in the surrounding tissues of the kidney, not involving the organ itself; and so far primary. 2nd. Those cases where the phlegmon is the result of an affection of the kidney itself, and where the antecedent pathological changes furnish sufficiently the necessary requisitions for the diagnosis, such as nephritic colic, calculus, nephritis, pyelo-nephritis, abscess of the kidney, and with these renal symptoms the further evidence of pus in the urine; such cases are secondary or consecutive perinephritic abscesses.

In the present review we intend to draw attention to the first class of cases only, and merely take a passing glance at the second variety.

The cases we have arranged in a tabulated form, and only leading facts are mentioned, so that our readers must refer to the original sources for more accurate information.

A.—*Primitive Perinephritic Abscess.*

No.	Sex.	Age.	Cause.	Peculiarity of symptoms.	Treatment.	Progress.	Result.	Reference.
1	M.	Labourer	Contusion, from fall, on loin; right side	Echymosis in loin, bloody urine, fever, rigors, swelling, fluctuation	Incision in lumbar region	Pus escaped, much phlegmon	C.	Bergonhioux; Troussseau, 'Clin. Med.,' vol. iii, p. 718, 1865.
2	F.	Nurse.	Injury, fell down eight steps; right side	Fever, vomiting, swelling in loin, had bloody urine, abdominal hardness, paralysis on same side, obscure fluctuation	Caustic potash over fluctuating point	Subsequent flow of pus, punctures made 3 weeks after caustic application, the abscess had reached to the umbilicus	C.	Trousseau; Bientait; Troussseau, op. cit., p. 718
3	F.	32	Not certain, insidious, jolting of carriage; right side	Intense pain in loin, swelling, wasting, vomiting, extension of swelling to iliac fossa, pain along psoas muscle, flexion of thigh	Incision in lumbar region; hemorrhage	Much pus evacuated, vessels tied, tent placed in wound	C.	Trousseau, op. cit., p. 714.
4	M.	Labourer	Muscular effort; left side	Pain, tumefaction, subsequent deep fluctuation, no albumen in urine	Incision in loin	Incision required enlarging in the course of a few days	C.	Tardieu; Troussseau, op. cit., p. 719.
5	F.	Lady	Vesical irritation for 2 years, uncertain; right	Pain in right flank for 8 days, violent fever, daily rigors, cedema of loin, fluctuation detected; operation deferred	Leeches, cataplasms, incision in loin to some depth	Much pus escaped, the tumour in loin had rapidly increased, and swelling reached to umbilicus, pushing colon to one side	C.	McCarthy, Guérin; Troussseau, op. cit., p. 726.
6	M.	...	Removal of testicle, ligature of cord, wound healed, cancer testis; left side	Thirty-two days after operation, rigors, fever, vast abscess in left loin and extended to pelvis, nausea, query consecutive abscess	No operation	Death in 3 days, no pus found in the veins, was not pyemic, all organs in body were healthy, pus around kidney, extending down the pelvis	D.	Chopard; Troussseau, op. cit., p. 728.

7	M.	35	Fatigue in walking; left side	Pain in left loin, thought to be lumbago; no fever or rigor; 8 days after, tumour in loin with redness, circumscribed hardness	Incision not until 6 weeks, pus fetid, kept open by tent	Fifteen days later tumour more prominent, obscure deep fluctuation, no fever, no alteration in urine	C.	Cuvasse; Trousseau, op. cit., p. 730.
8	M.	34	Left side.	Vast phlegmon in left lumbar and dorsal regions, redness, pain, fever of 6 weeks' duration	Incision in loin; immense quantity of dirty pus	Suppuration and abscess extended to umbilicus and into external iliac fossa; the latter abscess burst spontaneously	C.	Trousseau, op. cit., p. 735.
9	M.	...	Operated on for lithotripsy successfully	Lumbar pains, thought to be ilio-lumbar neuralgia; tumour in loin, fluctuation	Free incision, deep, pus close to kidney	No vesical, urethral, or renal symptoms	C.	Nélaton; Trousseau, op. cit., p. 728.
10	F.	17	No cause; right side	Pain in loin, swelling, fluctuation; 3 weeks	Incision	Evacuation of pus, probe entered 3 inches, remained open 1 month, sinus healed up	C.	Bowditch, Case 1; Boston City Hosp. Reports, vol. i, page 27, 1870.
11	M.	5	No cause; left side	Ditto, ditto; no pus in urine	Incision	On 15th and 19th days after, pus in urine; none on the 36th day	C.	Ditto, Case 2; op. cit., p. 29.
12	F.	23	Insidious; right side	Pain and swelling in loin, no distinct fluctuation, urine natural, pleurisy	Exploration by trocar; no pus	Three days after operation, discharge of pus, and subsequently	C.	Ditto, Case 3; op. cit., p. 30.
13	M.	28	No cause; right side	Rounded swelling in renal region, pain in iliac fossa, no fluctuation, urine normal	Exploration by trocar; no pus	Subsequent discharge of pus through opening	C.	Ditto, Case 9; op. cit., p. 53.
14	F.	46	Debility, in bed 2½ mos.; right side	Obscure symptoms in loin over 7 months, swelling in loin, subsequently pus passed in urine	Trocar used, and pus evacuated	Four months after, orifice contracted, when seton introduced; pus in urine subsided, a fistulous opening remained	C.	Ditto, Case 5; op. cit., p. 41.
15	M.	44	No cause; right side	Pain in right loin, extending to groin, swelling, fluctuation, urine natural	Incision through lumbar fascia; no hæmorrhage	Half pint pus evacuated; 1 week after operation, pus in urine, which disappeared in 18 days	C.	Bryant, Guy's Hospital.

A.—*Primitive Perinephritic Abscess* (continued).

No.	Sex.	Age.	Cause.	Peculiarity of symptom.	Treatment.	Progress.	Result.	Reference.
16	Perinephritic abscess	No operation	Emptied into colon	D.	Parmentier & Crunveilhier; Troussseau, op. cit., p. 735.
17	Extensive phlegmonous inflammation of loin, suppuration, emphysema, pus in faeces	Incision in loin; pus and fetid gas	Abscess had opened into intestinal canal, escape of fecal matter through wound	...	Troussseau, op. cit., p. 737.
18	Similar case, emphysema extended to dorsal region, pus in faeces	Incision in loins; pus and gas	Abscess had opened into the colon	...	Ditto, op. cit., p. 737.
19	M.	28	Fell 30 feet on face and hands; left side	Intense pains in loin, bloody urine, induration in lumbar and hypogastric regions, pus from rectum, supposed to be psoas abscess	No operation	Large perinephritic abscess opening into intestinal canal; had pneumonia and phthisis	D.	Bowditch, Case 10; op. cit., p. 64.
20	M.	27	Fatigue in digging in garden; right side	Pain in loin, deep-seated non-fluctuating tumour, became prominent	Incision in loin; much hæmorrhage	At first simulated ilio-cæcal abscess	C.	Ditto, Case 7; op. cit., p. 50.
21	Boy	...	Right side	Symptoms at first abdominal, then entirely thoracic, perinephritic abscess entirely masked	No operation	Had pleurisy and empyema, thoracentesis with little benefit, evacuation of pus by lung, large perinephritic abscess found, which had made its way into lung	D.	Ditto, Case 4; op. cit., p. 36.
22	M.	50	Exertion as gardener; right side	Sudden pain in loin, subsided, thickening of parts about loin, severe thoracic symptoms set in	No operation	Long continued thoracic symptoms, pus through lungs, supposed phthisis, considered to be perinephritic abscess bursting into lung	R.	Ditto, Case 6; op. cit., p. 47.

23	Clerk	...	Supposed phthisis, un- certain; right side	Lameness in hip, rigors, fever, tenderness and fulness in loin, abscess burst into lung, tumour in loin fluctuated and pointed Perinephritic abscess	Incision in loin; pus evacuated	Burst into lung previous to opera- tion	C.	Ditto, Case 8; op. cit., p. 51.
24	Spontaneous; no cause		No operation	Burst into peritoneum	D.	Trousseau, op. cit., p. 735.
25	F.	..	Puerperal; right side	Phlegmon and suppuration of right broad ligament burst into bladder and vagina, 15 days after, inflam- mation in loin and fluctuation— query consecutive	Incision in loin	Extensive abscess formed in iliac fossa, requiring opening; hectic diarrhea; death; no post-mortem	D.	Ditto, op. cit., p. 723.
26	Both sides	Double perinephritic abscess	No operation	Left, passed into pelvis and emptied into vagina and bladder; right terminated in resolution	C.	Ditto, op. cit., p. 735.
27	M.	30	Passed calculi 4 years ago, pyelo-nephri- tis; right side	Lumbar pains 4 or 5 years, con- tinued large tumour in lumbar and iliac regions, and over liver, no œdema, fluctuation—query con- secutive	Caustic potash application	Abscess opened, much pus, great relief, able to get about, whence sudden peritonitis; abscess around kidney, not communicating with peritoneum	D.	Denarquay; Trousseau, op. cit., p. 738.
28	F.	26	No injury; left side	Sudden pain in loin, rigor, depres- sion, dragging of leg, urine nor- mal, no evidence of kidney disease, large abscess formed in loin	No operation, although sug- gested	Cessation of symptoms, pus in urine, tumour in loin prominent and fluctuating, abscess burst into colon and communicated with pelvis of kidney; abscess involved upper and back part of kidney	D.	Hughes; 'Path. Trans.,' vol. ii, p. 235.

In reviewing the above Table, we find the following facts observed:

1. *The Sex*.—8 females, 15 males, and 5 not stated.

2. *The Age*.—In 8 cases there is no reference, and in 14 the following ages are given:—5, 17, 23, 26, 27, 28, 28, 30, 32, 34, 35, 44, 46, 50; and the remaining 6 are thus described:—a boy, a nurse, a lady, a clerk, and 2 labourers.

3. *Causes*.—Falls and contusions, 3 cases; jolting of carriage, 1 case; fatigue in walking, 1 case; muscular efforts, digging, in 3 cases; debility, 1 case; uncertain and insidious, 4 cases; no cause assignable, 6 cases; not stated, 6 cases. In 3 cases there is a doubt as to their being strictly primitive abscesses, but rather consecutive, viz. No. 6, where it occurred thirty-two days after the removal of a testicle, and there were no evidences of pyæmia; in No. 25, where it occurred fifteen days after an abscess of the broad ligament and had burst into the vagina and bladder; and in No. 27, where calculi had been passed four years previously, and he had suffered latterly from pyelo-nephritis.

In 15 cases the abscess was on the right side, in 7 on the left, in 1 on both sides; in 5 not mentioned.

4. *Complications*.—16 cases in which there was no complication present; in 5 cases the abscess opened into the colon; in 1 case into the peritoneum; in 2 cases into the vagina and bladder; in 3 cases it burst into the lung; and in 3 cases there was pus in the urine, viz. Cases 11 and 15, both having normal urine at the time of the abscess, which was freely laid open, but in both cases pus was passed by the urine, in the one instance after the first week, and in the other after the second week; both recovered, the urine becoming natural. The third case, No. 14, the abscess had probably burst into the pelvis of the kidney before operative measures were adopted; the pus in the urine did not subside until some months after the operation.

The condition of the urine has not been very carefully recorded in the cases generally, but in 7 cases it has been described as normal; and as the examples are cited as being free from renal complication, the urine may be presumed to be natural.

5. *Treatment*.—In 8 cases there was no operation; of these in 1 the abscess burst into the bladder and vagina, with recovery; in 1 it burst into the lung, and with relief; in the remaining 6 cases death ensued, the abscess bursting into the colon in 2 cases, into the lung in 2 cases, into the peritoneum in 1 case, and in 1 it passed down into the pelvis.

In 2 cases the caustic potash was used as a means of opening the abscess.

In 3 cases the trocar and canula was the means employed, but only effectually in one of these cases.

In 15 cases free incisions were used, and recovery ensued in all except one, where the patient died from diarrhœa, hectic, and exhaustion.

6. *The Termination*.—18 recoveries, 8 deaths, and 2 not mentioned.

Of the 8 deaths, 6 were cases in which no operation was performed, and death was mainly to be attributable to this circumstance. One man died from peritonitis, quite independent of the disease; and one woman died from hectic fever and diarrhœa. In both these cases the abscess had been freely opened.

We will now cursorily survey the symptoms which mark this disease, and we shall quote M. Trousseau's own words:—"In these cases of perinephritis, independent of all renal lesion, the patient suddenly, and from varied causes, complains of a deep-seated, diffused pain in the lumbar region, of an acute or dull character. This spontaneous pain, at times lancinating, is always increased by pressure, and more especially when the loin is grasped between the hands. The pain may sometimes disappear for several days, weeks, or even months, and then reappear in intensity. But this is not the usual course; for the suffering is generally persistent, and increases daily until the pus is evacuated. This pain is always a symptom of great importance, because it is the only local sign which may manifest itself for several days or weeks. Even at this time there exists general derangement of the constitution, showing that the suffering has an organic connection, there being symptoms of continued fever, in paroxysms, with rigors towards evening. The paroxysm is often very marked, and composed of three stages, analogous to the stages of an access of marsh intermittent fever. The patient is attacked daily with rigors of varied duration and intensity, succeeded by a stage of very marked heat and frequency of pulse; then the paroxysm terminates in abundant sweat. This fever soon weakens the patient; there is loss of appetite, wasting, and sometimes vomiting, on the onset of the fever, but there exists always obstinate constipation. This constipation may be due to the continued fever, but certainly also to the necessary aggravation of the lumbar pains in attempts at efforts at stool, the patient dreading the employment of any muscular exertion.

After a period varying from eight to fifteen days, in which the patient has had no other symptom than the local pains, general weakness, and fever in quotidian paroxysms, other local symptoms now begin to manifest themselves, viz. deep-seated phlegmasia; the region becomes more and more painful on pressure, and is the seat of a more or less doughy condition; at the same time the costo-iliac fossa is effaced, and if the patient be in the recumbent position, a more or less marked prominence may be felt by placing the hand over the lumbar region; it may also be apparent to the sight; by keeping one hand on the loin, and placing the other over the anterior surface of the abdomen, a deep-seated tumour will be detected between the two hands, and continuous with the cellular tissue. This tumour is immovable when the patient is requested to make full respirations, and one thus obtains a certainty that it is independent of the liver, which falls and rises at each inspiration and expiration. The doughy feel of the lumbar region is accompanied often with œdema, and this œdema may extend to the dorsal and gluteal regions; at the same time there may be a little redness of the skin.

This redness is erysipelatous in those cases where the inflammation extends to the cellular tissue of the region. Directly these local signs of inflammation exist, we may detect a fluctuation sufficiently distinct, but this fluctuation is almost always deep, requiring great experience in its recognisance; sometimes, however, it can only be suspected by the complication of the œdema, the doughy condition of the region, and the general symptoms. In fact, the moment pus has formed, there is increase of fever, the pulse becomes fuller and harder, and more incompressible, and the patient experiences frequent rigors. Under these circumstances the indication for interference is well marked, and one must not hesitate to give issue to the pus; for if this be deferred, the pus may pass into the iliac fossa, and even to the hip-joint, and thus may compromise the life of the patient.

Bowditch lays particular stress upon lameness and contraction of the thigh as a symptom in indication of the burrowing of the pus along the course of the *psoas* muscle.

Trousseau next directs our attention to the termination and complications of these abscesses. These we have already alluded to, and may be readily recognised on referring to the table.

The reviewer of Dr. Bowditch's paper gives a very succinct account of the results. He says:—"The results may be various. The inflammation may undergo resolution, but most generally it advances to suppuration, forming an abscess which may point externally at the loin, or it may burrow down the *psoas* muscle to the groin, or pass along to the gluteal region; it may also perforate the bladder, vagina, and rarely the colon. In some the pus advances upwards, along the course of the diaphragm, into the pleural cavity, or, if adhesions exist, into the bronchi. The thoracic complications are the most frequent, coming on in an insidious and latent manner; they should always be sought for, even when the symptoms point exclusively to the abdomen, because the fact of the existence of abnormal thoracic signs has an important bearing on the treatment and prognosis of every case of perinephritic abscess. On the other hand, the thoracic symptoms may completely mask the renal. In two of Dr. Bowditch's cases these were the sole symptoms; they masked all others, and had become in reality the most important, and alone called for treatment, and they were the chief cause of death."

Trousseau mentions two cases of perinephritic inflammation terminating in resolution, but neither of these are very satisfactory. Thus, at p. 720, he quotes a case of a young man, æt. 20, under the care of Dr. Bonin, who had after violent muscular efforts intense pain in the liver, followed by perinephritis and threatening suppuration; he was, however, suddenly cured, and all symptoms immediately disappeared after subcutaneous injection of sulphate of atropine. Trousseau considered that the patient never had perinephritis. The second case is related at p. 723, where a female, fifteen days after confinement, was attacked with rigors, fever, and inflammation of the broad ligament of the uterus, which ran on to suppuration and burst into the vagina. Soon afterwards a pain attacked the loin, followed

by swelling around the region of the right kidney; this swelling did not suppurate, but ended in resolution. The patient entirely recovered.

Trousseau bases the DIAGNOSIS upon the three important elements displayed by the disease, viz. the *pain*, the *swelling in the lumbar region*, and the *fever*, and he successively points out the characters displayed by the following diseases for which it may be mistaken, and from which it is to be distinguished. These diseases are:—typhoid fever, simple nephralgia, nephritis and pyelo-nephritic calculus, hydronephrosis and cancer, tumour of the liver, tumour of the spleen, perityphlitis, stercoral tumours of the large intestine, stercoral abscesses, and diaphragmitis.

He particularly cautions the surgeon to bear in mind that in all lumbar abscesses there is a possibility of the production of a hernia of the intestine.

The prognosis is generally favorable, provided the formation of pus is early detected and evacuated immediately; the danger consists in allowing the pus to make its way downwards into the pelvis, or upwards towards the diaphragm, pleura, and lungs, or inwards towards the peritoneum and intestines.

The treatment may in the early onset of the complaint be directed towards obtaining a resolution and arresting the pain—frictions of belladonna and opium, subcutaneous injections of morphia and atropine, cupping, leeching, &c. But when suppuration and fever have set in, operative measures must at once be had recourse to in order to set free the matter. Dr. Bowditch thinks that this may be done before actual distinct fluctuation is perceptible.

There are three modes recommended for adoption:

1. *Caustics*, to render the purulent collection more superficial and thus avoid hemorrhage;—these were employed in 2 cases—viz., 2 and 27, and with success, as far as regards their object.

2. *The use of the trocar and canula*, recommended by Dr. Bowditch and adopted by him in 3 cases—viz., 12, 13, and 14 in our Tables, but in only one did it succeed in evacuating the pus at once.

3. *Free incisions*, so strongly advocated by Trousseau;—cutting down in the lumbar region, external to the erector-spinae muscles, and on gaining sufficient depth, then the use of a director to tear the parts beneath until the pus is arrived at. Free incisions were used in 15 cases, and hemorrhage occurred only in 2 instances; the vessels divided are easily restrained from bleeding by torsion of the divided ends.

Where fistulous sinuses remain, astringent and iodine solutions may be thrown in.

We may fairly regard the laying open of the abscess by incision, and adopting the same measures and care as used in lumbo-enterotomy, to be the safest and most judicious method.

Consecutive perinephritic abscesses—are those which generally arise in the course of kidney and intestinal affections: they present the same symptoms as the primitive ones, plus the peculiar symptoms belonging to the renal and bowel affections, and the presence

B.—*Consecutive Perinephritic Abscess.*

No.	Sex.	Age.	Cause.	Progress of disease.	Operation.	Peculiarity of symptoms.	Result.	Reference.
29	M.	14	Abscess of pelvis of kidney	Pus in urine, swelling in loin, lameness of limb on that side	Punctured with bistoury	Fistulous opening remained; fresh abscess at ilium, burst and formed fistula; both fistulae subsequently laid open; drainage tube 9 days left in, and a fistulous opening remained	C.	Duffin, 'Medical Times and Gaz.,' Sept. 24, 1870.
30	F.	16	Abscess of kidney	Pus in urine, flexion of thigh, abscess in loin passed into pelvis, opened into colon, fresh abscess in loin	Incision into loin	Pus evacuated; some time after a fresh irritation lower down required	C.	Chuckerbutty, 'Lancet,' July 28, 1860.
31	M.	11	Calculus, pyelitis, abscess of kidney	Fluctuating tumour in loin	Opened into loin, spontaneously	Abscess passed through the lumbar muscles	D.	Browne, 'Path. Trans.,' vol. xiii, p. 131.
32	F.	Old	Infl. of gall-bladder and peritoneum, extending to tissue about kidney	Rigors and tumour formed in right loin	Opened by double puncture in loin	Subsidence of symptoms	C.	Trelat and Millard; Troussseau, op. cit., p. 732
33	M.	25	Chronic pyelo-nephritis	Suppuration, perinephritis	No operation	Perforation into pleura, pleurisy, empyema, thoracentesis; and pus evacuated	D.	Colin, 'Gaz. Hebdom.,' 1863, x, 40.

c.—Questionable Cases ; but quoted as Perinephritic by Dr. DUFFIN, 'Med. Times and Gaz.,' September, 1870.

No.	Sex.	Age.	Cause.	Peculiarity of symptoms.	Operation.	Progress of disease.	Result.	Reference.
34	F.	22	Abscess of kidney	Without bladder symptoms and urine normal during life, no reference made to the loin	No operation	Empyema, pus spat up in gushes, phthisis; post-mortem, kidney a bag of pus, pleura and lung involved	D.	Barelay, Dr., 'Lancet,' July 4, 1863.
35	M.	17	Tubercular kidney	Urinary symptoms; 3 years swelling in groin	Abscess burst in groin	Pus ran along psoas muscle, extending up to between pillars of the diaphragm	D.	Rees, G. O., Dr., 'Lancet,' July 4, 1863.
36	F.	43	Abscess of kidney	Renal disease for several years	Abscess pointed into groin	D.	Quin, Dr., 'Path. Trans.,' vol. v, p. 179.
37	M.	22	Tubercular abscess of kidney	Psoas abscess, flexion of thigh on pelvis	Abscess opened into groin	The abscess extended along psoas to the kidney	D.	Dickinson, 'Path. Trans.,' vol. xvi, p. 175.
38	M.	36	Abscess of kidney, on surface, under its capsule	Without bladder symptoms, urine normal during life; no lumbar signs	No operation	Gangrenous pneumonia, with softening of diaphragm, close to abscess, but no perforation, no pointing towards loin	D.	Gull, Dr., 'Lancet,' July 4, 1863.

of pus in the urine and fæces. The renal tumour or abscess may diminish in size every few days with coincident increase of pus in the urine and fæces. These cases are liable to the same complication and termination, and are to be treated by free and early openings.

We have given a table of a few examples as mentioned by Dr. Duffin; more might have been added, but we wished to confine ourselves to Trousseau's article. A third Table includes some questionable cases of perinephritic abscess, although there is no evidence on that point. Still the Tables B and C are useful addenda to Table A, as showing the great value of laying these abscesses open early, for out of these 10 instances, the only recoveries were those in which operative measures had been employed, being only 3 in number—Nos. 29, 30, and 32.

Analysis of 50 cases of Amputation of the Penis by means of the Galvano-cautery Loop Wire.

Dr. Zielewicz, of Breslau, fully enters into the subject of amputation of the penis and the diseases, for which it has been put in practice. He has confined himself to those cases in which the galvano-cautery was used as a means of removal, and enumerated several cases in detail. The following is a summary of the conclusions which he has arrived at:

1. The disease was for the most part carcinoma, in one case it was a colossal papillary growth, and in one gangrene of the organ.

2. Of the 50 cases there were 8 deaths, and all from pyæmia. The danger lies not only in the laxity of the cellular tissue of the penis, and in the veins of the neck of the bladder and about the prostate, which play an important rôle in the conveyance of purulent infection, but chiefly also to the hospital air, inasmuch as all the deaths occurred in hospitals, and none when performed in private.

3. Hæmorrhage did not occur in any of the cases, so far as the records show.

4. After amputation by the galvano-cautery, traumatic fever was totally absent.

5. The consecutive contraction of the urethral orifice was the same as met with in ordinary amputations.

6. The ages are recorded in 45 cases:—1 under 20 years, 6 between 30 and 40 years, 15 between 40 and 50 years, 15 between 50 and 60 years, 7 between 60 and 70 years, and 3 between 70 and 80 years.—*Langenbeck's Archiv f. Klin. Chir.*, 1870. Bd. 12, p. 589.

Remarks.—There have been in Guy's Hospital 5 cases of amputation of penis by the galvanic cautery wire during 1869 and 1870.

All were cases of epithelioma, and the ages were 42, 51, 56, 67, and 83. The duration of the disease previous to operation was in 1 case 2 months, in 2 cases 4 months, in 1 case 6 months, and in 1 case 2 years. In the latter instance excision of the growth had been performed in January, 1869, but the disease returned and was burnt down with the cautery in December, 1869; it again re-

turned, and the penis was amputated with the wire in October, 1870.

In no instance was there any hemorrhage, and all the cases recovered.

Mr. Bryant has removed a cancer of the penis, of 4 months' duration, from a private patient, æt. 72. Not a drop of blood followed the galvanic wire cautery. In this case he slit up the urethra and turned the margins back after the amputation. The old gentleman was quite well two years afterwards, having a good urethral orifice.

REPORT ON OBSTETRICS AND GYNÆCOLOGY.

By ROBERT BARNES, M.D. LOND., F.R.C.P.

I.—THE NON-PREGNANT STATE.

1. *Observations on the Innervation of the Uterus.* By Dr. REIMANN.
2. *Early Puberty.* By ROBERT HARRIS, M.D.
3. *Decidua Menstrualis.* By AL. SOLOWIEFF.
4. *On Sims' Doctrine of the Causes and Treatment of Sterility.* By SCANZONI.
5. *On Pyometra and Pyokolpos lateralis.* By BREISKY.
6. *On Sarcoma of the Uterus.* By Dr. HEGAR.
7. *Dermoid Cysts.* By Professor GLUGE.
8. *Dropsy of the Fallopian Tube.* By Dr. PEASLEE.
9. *Extirpation of the Inverted Uterus by Caustic Ligature.* By Dr. VALETTE.

1. Dr. Reimann has made some experiments illustrating the innervation of the uterus. He removed the uterus from the bodies of bitches and cats, and found he could produce movements in the uterus or Fallopian tubes under irritation by electricity, heat, cold, mechanical teasing, just as is observed in the heart. The movements were rhythmical. He concludes that the uterus possesses nerves independent of the cerebro-spinal axis.—*Arch. f. Gynäk.*, 1871.

2. Dr. Harris gives a contribution to the history of early puberty. A girl commenced to menstruate at nine years five months. The mammæ were well developed, hair upon the pubes, pelvis moderately full. A sister, æt. 12, had no sign of approaching puberty. As an example of the effect of heat over the menstrual function, he relates the following:—A young lady went to him, from a hot country, to be treated for menorrhagia. Staying in Philadelphia, the menorrhagia ceased; on going home it returned, so that she resolved to live in the north, when she entirely recovered.—*Amer. Journ. of Obst.*, 1871.

3. Dr. Solowieff details an interesting case of inflammatory dysmenorrhœa, in which the membranes expelled were carefully ex-

amined. The subject, æt. 21, married at 16½. She had begun to menstruate at 11, and had always suffered greatly. She had a child a year after marriage, and had to give up suckling in eight months. The first period after weaning was painless; but the following periods were marked by sense of weight and pain in the upright posture. Then the membranes were shed. The vagina was excessively sensitive. The periods were generally one to two weeks behind time, and a mass was expelled. Other symptoms appeared—extreme irritability, neuralgia, hyperæsthesia of the skin, especially of the belly. These were worse as the periods came on. Touching the roof the vagina called forth a series of reflex hysterical symptoms. Iodine injections effected little. The most benefit was gained from intra-uterine injections of perchloride of iron; and at last the membranes ceased to be cast. Solowieff describes the anatomical characters of the casts, and gives a theory of the mode of formation and expulsion. The membrane consists of three strata—(1) a fibrinous layer; (2) a glandular layer; (3) a granulation layer. The fibrinous layer is the outermost, *i.e.* that which was nearest to the uterine wall. The fibres enclose blood-corpuscles, red, and sometimes white ones. This layer passes gradually into the glandular layer, which contains the constituent parts of the glands, showing cylinder epithelium and numerous round cells. This layer passes gradually into the granulation layer, which shows a crowd of new formed vessels blocked with blood-corpuscles, giving a dark-red colour to the naked eye. The granulation tissue consists of round and long protoplasm-cells, with large nucleus. The cells lay in a soft, somewhat fibrinous, intermediate substance. Solowieff has never seen epithelium on the surface. The membrane is cast in the following manner:—During menstruation, successive effusions of blood take place, the blood, oozing between the cells, gradually loosens a part of the mucous membrane. This process does not take place suddenly, but the mucous membrane gradually sinks lower and lower, so that the blood-spot is formed at the fundus uteri. As soon as the blood has overcome all resistance, the decidua menstrualis, now a foreign body, is expelled in the form of a sac, surrounded with coagulated blood. The shape is due to the granulation layer; this gives thickness to the membrane.

As to the interesting question whether these membranes are necessarily dependent on impregnation, Solowieff thinks the present case is evidence that they may be independent.—*Arch. f. Gynäk.*, 1871.

4. Scanzoni discusses the recent doctrines as to the causes and treatment of sterility, especially Marion Sims' views. He insists that far too exclusive importance is attached to the mechanical hindrances to the meeting of the semen and ova. He says we know little as yet as to the influence of various morbid conditions upon the fertility of the semen and ova. Diseases of the testicle, it is known, sometimes lead to absence of spermatozoa. May not, he asks, the frequent diseases of the ovaries lead to the production of diseased or defective ova? Manifold experience proves that during

extreme anæmia conception does not take place. Here is a proof that in the case of the ovaries, as in that of other glands, a bad condition of the blood leads to bad secretions—ova incapable of fructification are produced. This defective knowledge of the pathological changes of the seminal fluid and of the ovum is the most important hindrance to a scientific foundation for the etiology and therapeia of sterility.

Another series of difficulties arises when we consider the indispensable locomotion of the semen and of the ovum. It is only necessary to call to mind the frequent abnormalities of the Fallopian tubes met with in autopsies—such as congenital or acquired shortenings, dislocations, adhesions—which are completely beyond clinical diagnosis. What do we know as to the condition of the muscular movement of the tubes, and as to that of the ciliary processes, or as to the medium in which the semen is received? Scanzoni then puts the case of a typical dysmenorrhœa with narrow os uteri and sterility. The os is split, the dysmenorrhœa is relieved, but the sterility continues. He asks, must it not be admitted that there is here a cause of sterility which lies in other and unknown conditions? (This may be granted, but the relief of the dysmenorrhœa is alone a sufficient reason for the operation; and besides, the narrow os being in all probability *one* cause of the sterility, it is perfectly logical to remove this cause, giving the patient the possible benefit of its being the only cause. Sound clinical reasoning dictates that we should eliminate all the known complications of a morbid state, and not leave them to harass a patient because there may be others beyond which we cannot relieve.—R. B.)

Scanzoni goes on to make various objections to prove that the narrowing of the os uteri is not clearly established as a sufficient or frequent cause of sterility. Thus, how often do we find difficulty in passing the sound through some part of the cervical canal, and yet conception taking place. He cites a case in which conception ensued with a typical conoid cervix with small os, in which no treatment had been used. He says that, after the most careful examination, he has not once been able to satisfy himself that sterility was solely due to an obstruction to the passage of the semen through the cervical canal.—*Scanzoni's Beiträge*, 1870.

5. Dr. Breisky gives a case of double uterus, in which blood was retained in one half. The subject began to suffer from uterine colics every four weeks when sixteen years old, no discharge appearing. The pain was most severe in the right side, and the abdomen became gradually larger after every period. Constipation and extreme anæmia followed; then difficult micturition. Suddenly she felt something burst, and a quantity of pale-red, thick, stinking fluid escaped, to her great relief. Discharge returned irregularly during a year, at times like thin pus. The difficult micturition returned. Breisky then punctured by the side of the os uteri, and let out a quantity of pus. He considers that the seat of the abscess was the right uterus.—*Arch.f. Gynak.*, 1871.

6. Dr. Hegar describes sarcoma of the uterus. He submits that

this morbid growth is more common than is generally believed. It occurs in two principal forms—(1) There are multiple tumours more or less distinct; (2) there is a diffuse infiltration. In situation and relation to the uterine wall they present a great likeness to the intramural fibrous tumours. The seat is almost always the body of the uterus. Veit's case, in which the cervix was affected, is unique. For the most part the sarcomatous tumours repose, with a broad basis, upon the inner surface of the fundus and body, projecting into the enlarged cavity, as not seldom do the intramural fibro-myomas. A kind of stalk is rare. Sometimes, as West says, together with the intra-uterine tumours, there is a second division, which develops itself towards the abdominal cavity, either in the iliac fossa or in Douglas's pouch, reaching even to the lumbar region. The intra-uterine tumour seldom possesses a distinct investment, but a thick, smooth, fibrous capsule may exist. Invasion of the new formation into the cervix may take place. Either a uniform infiltration of both walls, or of one wall and lip, with or without ulceration of the os uteri or tumours connected with those of the body, spring from the inner surface of the neck, or, lastly, larger tumours proceeding from one lip fill, like monstrous hypertrophies, the vagina.

In a second order of cases the sarcoma presents itself as a diffuse infiltration of the mucous membrane, submucous membrane, and even of the proper muscular wall. There is on the inner surface of the uterus a large ulcerated surface, with fungous granulations, necrotic débris of the original tissues and of the new formation hanging to it. Other parts show better preserved, but also rough, wart-like elevations, knobs, or polypous excrescences. The diffuse infiltration may penetrate all the tissue strata, so that the new growth presents the aspect of an enormous uterus, retaining its ordinary shape. The two kinds, distinct and diffuse, are at times blended. It is remarkable that apparently sound parenchyma is at times disseminated through the diseased tissue, recognised as foci by the microscope. The first origin of the degeneration is commonly sought in the mucous membrane, proceeding from whence the submucous and muscular tissue is invaded. The colour of the tumour is usually greyish-white, even white, but sometimes reddish-grey. The consistency differs. The tumour is soft, so that it breaks down under the fingers or the polypus forceps, like brain-substance or wetmortar. It may, however, be denser, like a soft or even a hard myoma. The round-cell sarcoma and the medullary form are most frequently found. The cells are either of medium size or small. The basis-substance is homogeneous, finely striped, not seldom delicately netted, as in gliosarcoma. Virchow saw distinct myxosarcoma. The spindle-cell sarcomas appear to be more rare. Combinations of round and spindle-cells are more common. When a larger proportion of fibrous connective tissue enters into the constitution of the tumour it acquires a firmer consistency. The proportion of fibrous connective tissue may be so great that we are compelled to admit a transition form, a fibro- and myo-sarcoma. In some cases the fibrous tissue is so preponderant that only very careful investigation can detect the significant differ-

ent tissue. Such mixed tumours may have existed from the beginning; but a sarcomatous proliferation may spread through a groundwork of fibro-myoma. Virchow speaks of proliferation process of myxomatous character into the rich and extensive interstitial tissue of many myxomas. Numerous vessels penetrate the structure of many sarcomas. The concurrence of carcinomatous and sarcomatous tumours is less common than the transition forms and mixed forms of myofibromas and sarcomas. Secondary deposits of the sarcoma in distant organs are seldom mentioned, as in the lungs, the parietal pericardium; they only appear after long duration of the disease. Deposits in neighbouring organs are somewhat more frequent, as extension of the degeneration to the vagina, to the rectum, with consecutive stricture and ileus. Deposits in the lumbar and retroperitoneal glands occur.

The etiology is obscure. It has been met with in puberty and in the climacteric period, in virgins and in women who have borne children. The previous health has been described as excellent. But in a small number of cases distress pointing to antecedent disease of the sexual organs has been complained of, as dysmenorrhœa, irregular frequent protracted menstruation, discharges, rather watery than sanguineous, leucorrhœa, abortions, sterility, and sometimes hysterical and nervous habits.

One of the earliest signs of the existence of the tumour is menorrhagia. This is very often attended by irregular or persistent hæmorrhages. Offensive mucous discharge was in one case the first symptom, being followed by bleeding. Peculiar hæmorrhages, and an offensive puriform or flesh-water like discharge, are rarely absent. These discharges may resemble those of carcinoma of the cervix, but the odour is generally less penetrating. At a later stage the discharges contain numerous small and larger shreds of the tumour. Sometimes a polypoid projecting portion is cast off, or falls into purulent degeneration, giving rise to an intolerable stench. To these discharges are sometimes added as early symptoms a sensation of pressure, of bearing-down pain in the sacrum, in the pelvis, pressure on the rectum, dysuria. But Hegar does not agree with Gusserow in admitting that pain is constant and early. Mostly the pains possess a labour-like character, and point to real contractions. Besides these principal symptoms, there are numerous so-called consensual symptoms, such as attend the most different diseases of the sexual organs, especially disorders of digestion, cardialgia, &c., nervous symptoms, nutrition suffers; from dysuria, hydronephrosis may follow. The constant issue is death, which either ensues from exhaustion through the protracted discharges, through pyæmia, or the consecutive diseases.

Objective symptoms in the living vary according to the anatomical relations of the new growth. Through the abdominal walls we may feel tumours of varying size, knobby, overlapping, or distinct, between which the body of the uterus may be made out or lost in the imbedded tumours. If, in the case of an intra-uterine tumour, rapid degeneration does not ensue, the cervix gradually dilates, its wall

softens, its lips disappear, the os uteri opens, and the tumour is driven through, perhaps into the vagina, stimulating a fibrous polypus, when it becomes gangrenous, suppurative, and is cast off in larger or smaller pieces. The sound generally indicates an enlarged cavity of the body of the uterus, and strikes upon rough irregular places. The mobility of the uterus is often retained for a long time. At a late stage peritonitis, proceeding from the extension of the disease to the neighbouring organs, may fix the organ.

The diagnosis is generally clear; but the microscope alone makes it certain. It is easy to get small portions of the tumour. Simple hyperplastic growths are distinguished by their fibrous connective-tissue muscular substance, vessels, follicles, glands.

The duration of the disease is variable, from five months to six years. But it lasts longer than carcinoma of the cervix. The treatment is simple. If the cervix is not dilated, it must be dilated, so that the finger may reach the basis of the tumour, and permit its removal by scissors or polypus forceps. Generally hooks tear out, and will not hold. After the operation, Hegar has introduced *potassa fusa*; a copious discharge followed, bringing away shreds of the tumour.

Hegar concludes by relating eight cases.—*Arch. f. Gynäk.*, 1871.

7. Professor Gluge relates (*Presse Méd.*, 1870) a case from the practice of Dr. Grégoire, in which a dermoid cyst was spontaneously expelled from the urethra. The patient, *æt.* 30, had complained of retention of urine. A cyst, with some little bones and hair, came from the urethra. There were signs of inflammation of the bladder, but the woman recovered.

8. Dr. Peaslee relates an interesting case of dropsy of the Fallopian tube. The patient had been tapped twice for ovarian dropsy. After death there was found on the right a true ovarian cyst; and on the left a tumour of the Fallopian tube of very large size. The tube had become occluded at the very commencement of the uterus; accumulation took place beyond, until the tube itself was distended into a sac, with the capacity of eighteen pounds. The whole was adherent to everything in its neighbourhood.—*New York Med. Journ.*, 1870.

9. Dr. Valette discusses the methods of amputating the inverted uterus by the knife, by *écraseur*, by slow ligature, and describes a case in which he removed the uterus by caustic ligature. His apparatus consisted of a kind of double clamp, each branch of which was hollowed on its internal aspect, so as to receive a roll of chloride of zinc paste. The neck of the uterus was seized in this clamp; and to prevent the retreat of the stump four pins were made to traverse it; the neck was then divided in front of the clamp by a knife; then some chloride of zinc was applied to the surface of the stump. The part cicatrised well; recovery was perfect.—*Lyon Médical*, 1871.

II.—PREGNANCY.

1. *On the Morphology of the Milk-Casein.* By Dr. KEHRER.
2. *Daily Observations on the Change of Position and Presentation of the Fœtus in the latter months of Gestation.* By Dr. HOENING.
3. *Practice in the Predictions of the Day of Confinement.* By Dr. MATTHEWS DUNCAN.
4. *Tubal Gestation complicated with Ovarian Cyst.* By Dr. CHIROBAK.
5. *A case of Inguinal Hysterocele, Pregnancy supervening.* By Dr. SCANZONI.

1. Dr. Kehrer has studied the conditions of production of the milk. He enunciates the following propositions:—1. The gland-cells of the mammæ undergo active division throughout the stage of milk-preparation, and, on the other hand, break up after fatty metamorphosis into fat-globules and irregularly shaped protoplasmic débris. 2. The fat-globules of the milk are not surrounded by albuminous or caseous investments. 3. The cell débris (interglobular substance) spring up in the milk serum, and form with it a thin sinew. 4. This mucus is the emulsion of the fat-globules. 5. In fresh milk the new cell-débris are invisible; in coagulation they appear in the form of nuclei and nucleus-bearing flakes. 6. They collect together out of a light basement substance and nucleated coagulating casein. 7. The casein is not dissolved either in the water or in the salts of the milk, but is contained in it as a constituent of formed particles.—*Arch. f. Gynäk.*, 1871.

2. Dr. Hoening has conducted an extensive series of observations on the change of position of the fœtus during the latter months of pregnancy. His plan was to make daily observations. Amongst his conclusions are:—1. The stability, that is, the non-liability to change, of the head presentations greatly exceeds that of all the other presentations, especially in primiparæ. Breech presentations are more stable in primiparæ than in pluriparæ. Oblique presentations, on the other hand, are more stable in multiparæ. 2. Pelvic contraction is of great influence over the frequency of change; changes of presentation are three times more frequent than in normal pelvis. 3. It was not observed that the age of the mother had any influence. 4. The heavier the fœtus, the less frequent was change. A head presentation frequently changes to a different position, that is, a first becomes a second, and *vice versâ*, but change from a head presentation to an oblique or breech presentation is rare.—*Scanzoni's Beiträge zur Geburtsh.*, 1870.

3. Dr. Duncan, criticising Dr. Ahlfeld's memoir on the duration of pregnancy, says calculation must be empirical. He takes the following method:—Find the day on which the woman ceased to menstruate. Take that day nine months forwards as 275 days, unless February is included, when it is taken as 273 days. To this add three days in the former case, or five if February is in the count, to make up the 278 days. He tests this by 153 cases. The predic-

tion should not state the week, but the fortnight of delivery.—*Edin. Med. Journ.*, 1871.

4. Dr. R. Chrobak relates a case of tubal gestation complicated with an ovarian cyst. A widow, æt. 47, pluripara, observed in spring, 1867, increase of menstrual bleeding, and swelling of the lower abdomen. Pains attended the three-weekly menstrual discharge. Oppolzer found a tumour, slightly movable, hard, where it stretched to the right and outwards, and tense elastic, where it extended to the left. The uterine cavity measured $4\frac{3}{4}$ ". Hæmorrhage followed. Incision was made in the neck of the uterus, fibroid being diagnosed. Hæctic set in, and death followed anæmia and peritonitis. Left ovary converted into a tumour, size of a child's head, thick-walled, and filled with mucus; on its outer wall was a daughter cyst. The left tube was attached to the outer end of the cystic tumour; it contained a sac the size of an apple, also adherent to the cyst. It had burst. The right ovary contained a Graafian follicle very distinct. Chrobak thinks there was in all probability transmigration of the ovum from this ovary across the fundus uteri to the left tube. (It is not shown, however, that there was not, or might not have been at the time of impregnation, a portion of the left ovary capable of producing ova, as has often been observed in cystic disease. R. B.)—*Wien. Med. Presse*, 1870.

5. Scanzoni relates in detail a most interesting case of inguinal hernia, in which the contents of the sac consisted of the uterus and ovaries, pregnancy twice supervening whilst the uterus was in this position. The physiological observations made are of special interest. The subject first menstruated at twenty-one. The function ceased; she suffered from ascites and œdema. From twenty-two menstruation became regular; when twenty-eight she conceived; labour came at normal term; was very protracted; child alive. She had a second child, and returned to work three days afterwards. Four weeks later, lifting a heavy wine-cask, in doing which she fell, she remarked a swelling in the left groin, which could not be reduced. This state lasted for four years. Then she had typhoid, during which the menses ceased, and profuse leucorrhœa set in, whilst the inguinal tumour reached in a short time the size of a man's fist. Examined, there was found a swelling, smooth and firm, extending towards the left labium magnum. It contained a large tumour, broader below, becoming smaller above, flattened from before backwards, and more sensitive on pressure than the surrounding parts; outwards and above this larger pear-shaped body was a smaller body the size of an apricot kernel; this was less movable, but generally much more sensitive, and became distinctly more painful and swollen during menstruation. By vaginal examination the vagina was found elongated, dragged upwards and to the left, narrowed to a funnel; the vaginal roof thus dragged up revealed no trace of vaginal portion of uterus, but a small round opening. Whilst the tip of finger is in this opening, if the body in the hernial sac is lifted up, so as to press it over the horizontal ramus of the pubes, it is plainly felt that the dragging of the vagina towards the left is

slackened, and that a cicatrised vaginal portion sinks into the vagina. A fine sound was once passed through the os uteri all along the inguinal tumour quite to the fundus. A sound passed into the bladder, and a finger in the rectum, established that the uterus was missing from its proper place. Two years later the tumour increased in size after lifting a weight. Two years later still she married, immediately conceived, and aborted in two months. No marked hæmorrhage followed. The tumour, which immediately before the abortion was as big as two fists, quickly shrank to its former size. Eight and a half months after this abortion signs of pregnancy appeared. The tumour became troublesome from its weight, and later, acute, at times remitting, stabbing, lancinating pains appeared in it. Painful micturition, and urgent call to pass urine, and obstinate constipation. When about four to five months gone, the uterine sound was heard, the foetal heart not. The vagina was remarkably long, dragged to the left. Palliative treatment until, the patient being nearly five months gone, the pains became almost unbearable, and accompanied by febrile movement. The tumours became red, enlarged almost visibly; the skin showed intense filling of the subcutaneous veins, and looked shining and stretched. Induction of abortion was decided on. A stilet was passed into the uterus, and warm water injected. A dead foetus was expelled. The placenta followed spontaneously. For some hours after labour the patient complained of severe after-pains; but next day all fever was gone. The uterus was much less sensitive to touch than before labour. The foetus measured 8"6", weighed 75 grammes. The diminishing tumour was carefully measured. Before labour it measured 16 centimètres long, 44·5 centimètres in circumference; the day after it was 15 centimètres long, 38 centimètres in circumference. The diminution went on steadily from day to day, until on the seventh day the length was 12 centimètres, circumference 30·5 centimètres. At this point it was stationary for three days, when after two doses of ergot the tumour again began to diminish; the very next day it was 10·5 centimètres long, and 29·5 centimètres in circumference. On the twelfth day the length was 9 centimètres, the circumference 28 centimètres. Thus the loss was 7 centimètres in length, and 10 centimètres in circumference, in eleven days. Further measurements could not be made, as the patient was well, and insisted on leaving hospital.—*Scanzoni's Beiträge*, 1870.

III.—LABOUR.

1. *On the Etiology of Habitual Abortion.* By Dr. OLSHAUSEN.
2. *On the Pathology and Treatment of Abortion.* By Dr. HOENING.
3. *The Determining Cause of Labour at Full Term.* By A. F. A. KING, M.D.
4. *A Contribution to the Study of Eclampsia: Ammonia in the Blood.* By Dr. SPIEGELBERG.
5. *Two Cases of Convulsions successfully treated by Chloroform.* By Dr. E. R. TOWNSEND.
6. *Convulsions treated by Chloroform,* Dr. WILTSHIRE; *by Chloral,*

- Dr. VON SEYDEWITZ; *by subcutaneous injection of Aconite and Morphia*, Dr. BOWSTEAD.
7. *Contraction of the Pelvis*. By Dr. MARCHANT.
 8. *Kyphotic (transversely contracted) Pelvis*. By F. SCHATZ, KLEIN-WÄCHTER, CHANTREUIL, LANGE.
 9. *A new kind of Pelvis, capable both of Dilatation and of Narrowing*. By Dr. WINKLER.
 10. *Pelvic Tumours as Obstruction to Labour*. By Dr. HABIT.
 11. *Fibrous Tumour of the Uterus and Pregnancy; two cases*. By Dr. L. SEDGWICK.
 12. *On Ruptures between the Clitoris and Meatus Urinarius during Labour*. By Dr. P. MÜLLER.
 13. *The Management of the Perinæum during Labour*. By Dr. GOODELL.
 14. *On the Mechanism of the Expulsion of the Placenta*. By Dr. MATTHEWS DUNCAN.
 15. *Rupture of the Uterus*.
 16. *Decollation of the Fœtus*. By Dr. HYERNAUX.
 17. *On the Use of Sutures to the Uterine Wound after Cæsarian Section*. By Dr. RODENSTEIN.
 18. *On Cæsarian Section post-partum*. By Dr. HOSCHEK.
 19. *A case of Cæsarian Section*. By Dr. J. W. ROE.

1. Dr. Olshausen says there are two chief causes of habitual abortion: syphilis and retroflexion of the uterus. Abortion in the latter half of pregnancy is generally from the first cause; abortion in the early months from the second.—*Berliner klin. Wehnschr.*, 1871.

2. Dr. Hoening, in a memoir on abortion, relates an interesting case in which an early abortion took place, repeated hæmorrhage followed, rigors, hectic; the bleeding stopped for awhile, then returned, as well as the hectic, and great prostration. Examined by Veit, the uterus was found as big as a child's head, and enclosing placental remains; at the right angle of the uterus, and behind, was a tumour the size of a small fist, not painful. There flowed from the uterus, besides blood, an irritating foul-smelling discharge. Temp. 39.5° C., pulse 120, resp. 24. The placental remains were found adhering chiefly to the right, corresponding to the site of the tumour. A portion was removed with considerable trouble. Hectic continued; temperature fluctuated between 37.2° and 40° . Large quantity of foul-smelling fluid escaped from the tumour, and lukewarm injections were made. Cough and consolidation of lung set in. Pain appeared in the region of the tumour extending all down the right leg. At the end of three weeks, the remains of the placenta was removed. Pain spread over the abdomen, and the bad odour of the discharge disappeared. Three days before death a thin extremely foul-smelling stool was passed; on the last day there was retention of urine.

Autopsy.—General peritonitis; all intestines adherent; liver soft, fatty; intestines adhere to uterus; uterus much enlarged; a per-

foration was found in its upper and posterior part, the edges of which were irregular and suppurating; in this region was a cavity formed by adhesions between uterus, omentum and intestines, containing some tissues advanced in puriform degradation, floating in discoloured pus. A communication was found between this cavity and the ileo-cæcum. The uterine cavity was empty; where the placenta had grown, was a hole opening into a cavity in the uterine substance which was filled with thrombi passing into purulent degeneration. The bladder was inflamed, and had also a perforation into the tumour.

Dr. Hoening, discussing treatment, describes a method of removing placental remains by expression. He says, in many cases, it is possible to drive out the contents of the uterus by the following manœuvre. Two fingers being in the vagina, the fingers of the other hand seize the body of the uterus through the abdominal wall, and then the uterus is squeezed between the two hands, the finger in the vagina being applied outside the uterus so as to make counter-pressure. When the uterus is anteverted the fingers applied to the abdominal wall get behind the uterus and press it firmly against the symphysis pubis. Dr. Hoening says this method will empty the uterus easily. (Unfortunately this method cannot answer when the decidua retains intimate adhesion to the uterus, as is often the case. One or two fingers then must be passed into the uterus.—R. B.) (*Scanzoni's Beiträge*, 1870.)

3. Dr. King, of Washington, discussing the theories put forward to explain why labour occurs at full term, submits the following propositions. When pregnancy occurs the uterus grows to keep pace with the growth of its contents, so that until the end of gestation there is a constant mutual adaptation between the capacity of the uterus and the bulk of its contents; but at full term a very opposite condition of things occurs: the womb having reached the limit of its development, ceases to grow, whilst the fœtus not only continues to grow, but seems to increase even faster than at any preceding period. Hence—the first determining cause of labour—the cause of “inherent,” “insensible,” peristaltic contractions—is distension of the uterus, this distension taking place as a necessary consequence of the womb having ceased to augment its capacity, while its contents continue to increase.—(*Amer. Journ of Obstetrics*, 1871.)

Dr. O. Spiegelberg has submitted to experiment *the theory of Frierichs*, which assigns to ammonia, as the result of decomposition of urea in the blood, the production of convulsions. A primipara was seized with convulsions during pregnancy. The urine was loaded with albumen. Blood was drawn twice. After labour the patient recovered. The blood was received in two vessels, and quickly secured against air. One specimen was examined for ammonia, the other for urea. The first gave by Kühne and Strauch's apparatus distinct evidence of ammonia. A quantitative analysis was not made. The other gave 0.055 per cent. of urea. The urine gave 4.78 per cent. of albumen, and 1.1 per cent. of urea. There

was thus excess of ammonia and of urea in the blood, and diminution of urea in the urine.

To this observation in the human subject, Spiegelberg adds some experiments made on dogs, to determine whether ammonia has the property of causing convulsion and coma when injected into the blood. Oppolzer had stated that carbonate of ammonia only acted as an irritant producing convulsions, not coma, whilst Petroff had confirmed the theory of Frerichs, that it was the cause of both. Spiegelberg's experiments consisted in throwing solutions of carbonate of ammonia into veins and arteries of dogs. They were four in number. The results went uniformly to prove the similarity of the symptoms, convulsive and comatose, with those observed in the human subject in uræmia. He concludes that in the case narrated there was ammoniacal blood-poisoning in the sense of Frerichs.—*Arch. f. Gynakol.*, 1870.

5. Two cases of puerperal *convulsions successfully treated by chloroform*. Dr. E. R. Townsend, jun., relates two which illustrates the action of chloroform in puerperal convulsions; but since the treatment was complicated it can hardly be said that the beneficial action of chloroform is proved. The first case, that of a lady in the seventh month of her fifth pregnancy, showed premonitory nervous symptoms. When convulsions set in, the os uteri was closed. Cold affusion, then ice to the head, was used. Sinapisms to the legs and stupes of hot water. Pulse rapid and feeble. Calomel on tongue. Coma set in. Labour was therefore induced, and accelerated by Barnes's bags, forceps, and craniotomy. The chloroform was not given until after delivery. The effect seemed to be that if given freely just at the first sign of twitching indicating the approach of a fit, the fit was not averted. The patient recovered.

In the second case, that of a weak half-starved woman, in labour at term of her first child, the fits came on during the first stage of labour. Albuminuria. Coma followed the fits. Chloroform was given and delivery effected by forceps. Hæmorrhage followed placenta. Ice was applied to the head. Chloroform was given so as to anticipate the full development of the fits. She had croton oil. The albumen disappeared. The patient recovered.—*Dub. Quar. Journ. of Med.*, 1871.

6. Dr. Wiltshire relates a case of severe convulsions setting in with labour; pulse 180, respirations 80 per minute; legs œdematous, urine highly albuminous; head presenting, membranes entire. She was put under chloroform; the cervix was dilated by Barnes's bags, and a dead child was delivered by forceps. Dr. Wiltshire is clear in his opinion that the fits were checked by the chloroform, and that by this and the acceleration of the labour the patient was saved.—*Practitioner*, 1870.

Dr. Von Seydewitz describes one case treated by chloral. The patient was epileptic, and fits came on some days after labour. Recovery followed.—*Obstetrical Transactions*, 1871.

Dr. Bowsted relates two cases in which subcutaneous injection of two minims of Fleming's tincture of aconite and one third of a grain of morphia seemed remarkably successful.

7. *Contraction of the pelvis* is elaborately discussed from a practical point by Dr. Marchant (*Journ. de Méd., &c., de Bruxelles*, 1870-71). When the smallest diameter measures at least 90 millimeters, labour may still be effected, although slowly. He examines the influence of each different presentation of the head in this degree of constriction. He gives cases, amongst them several in which the head, arrested at brim, was brought down by the lever, after the manner of Professor Fabbri. In the second degree, when the smallest diameter measures from 70 to 90 millimeters, where the relations are well determined, it is better not to wait long before giving assistance. It is well, however, to let the head be a little fixed in the brim first. The forceps or lever will overcome arrest generally when there is 80 millimeters. Marchant dwells upon the deviation of the axis of the pelvis, caused by the projection of the sacral promontory, and considers how the necessary backward traction by forceps, &c., can be effected. Some have curved the handle of the forceps again to obviate pressure upon the coccyx. Marchant, after Hubert, praises the lever as specially adapted to this case. The lever can be applied over the parietal region, and thus seizes the head in the most compressible direction; when under aid of traction, compression and expulsion, delivery is accomplished.

Herbiniaux, Boddaert, Coppée, Beytter, and other Belgian authorities are cited as having proved the value of this practice. When the head is movable above the brim, Fabbri advises turning. Marchant says, the forceps may still be tried when the contraction is at 70 millimeters, although the prospect is not good. He then discusses minutely the question of turning, disputing the objections of Joulin. He cites a remarkable memoir by Dr. Roger, from the '*Annales de la Soc. de Méd. de Gand.*' 1868, who gives 106 labour-histories of 25 women with pelvic contraction to 5 or 6 centimeters. The following were the results:—In 62 cases, version only was practised, giving 43 living children; the forceps, used 9 times, brought 9 dead children. In 9 cases labour was completed by turning, after forceps had failed, 3 children being saved.

When the smallest diameter is from 70 to 45 millimeters, here forceps, lever and turning are useless, and should not even be tried. Embryotomy or Cæsarian section offer the sole means. As to Cæsarian section, he says, the physician is bound to prefer embryotomy, if the mother demands it. He is bound to assist her. Perforation simple is insufficient below 65 millimeters. Three means of craniotomy exist, cephalotripsy, diaclasia, and sawing. He extols enthusiastically what may be called the Belgian method, that of Van Huvel by the forceps-saw. (But it is difficult to be carried away by this enthusiasm so far as to hope that the forceps-saw can ever be applied to extreme cases. The blades require *at least as much* room as the ordinary forceps, whilst a good cephalotribe requires less, and the reporter's wire-écraseur can work through the merest chink that will admit the finger to guide it.—R. B.) Marchant then discusses symphysiotomy, and argues in favour of it.

Dr. Hoening calls attention to the not infrequent projections

formed behind the symphysis pubis by the eversion of the ossa pubis at the line of junction. This mostly happens in rachitic subjects, and may lead to dystocia. Hennig relates three examples. In one, he says, the excrescence disappeared during childbed.

S. Dr. Schatz relates a case of transversely-contracted pelvis, arising from inflammation of the hip-joint. The subject was quite healthy until the age of fourteen, when she had inflammation and abscess of the right hip-joint. Chronic disease persisted for several years. Labour set in on 15th September; breech, assistance required, arms liberated with difficulty, head could not pass, the body was therefore separated in order to perforate the head, the cephalotribe was then applied, and the head delivered. On removing the placenta a considerable rupture on the left side of the uterus was detected. It was supposed this had been caused by the sharp end of the cervical vertebrae, where the head was turned round, and drawn down by the cephalotrib. Signs of traumatic peritonitis followed. A week afterwards a large quantity of pus flowed. Suppuration went on for three months, during which time a firm tumour was felt in the left hypogastrium. The pelvis was carefully measured and a figure is given. The conjugate diameter is 3·50", the transverse is under 3·00", the right side of the pelvis runs almost straight from the side of the promontory to the symphysis pubis.—*Arch. f. Gynäkol.*, 1870.

Dr. Kleinwächter relates a case of labour, with transversely contracted pelvis, in the Prague Hospital. The subject, æt. 27, primipara, at term, was in labour on the 18th December. The waters escaped on the 20th. The forceps had been tried before admission. The woman was well built; the bones, especially the pelvic, very thick. The pelvis seemed symmetrical. The trochanters were distant from each other 10·50". The width of the pubic arch at the level of the union of the ascending rami of ischia, and of descending rami of the pubic bones, was under 2·50"; the distance of the tubera ischia was 2·50"; the external conjugate was 10·75". It was therefore evident that the pelvis was transversely contracted. The patient, being much exhausted, was delivered after perforation by the cephalotribe. It was reported that, when a child eleven months old, she had been let fall, so that her sacrum fell upon the edge of a wheel. The woman recovered, but died some weeks later away from hospital, so that the pelvis, which Schatz thinks would have matched Robert's, was lost. He cites seven other cases, showing that, in all but two, Cæsarian section was necessary.—*Ibid.*

Dr. Chantreuil describes ('Gaz. Hebdomadaire,' 1870) two cases of *kyphotic pelvis*. The inlet was chiefly affected, the conjugate being increased.

Dr. Lange describes a peculiar case of *kyphotic transversely contracted pelvis*. The subject was thirty-four years old, of weak organisation, living in great poverty; learned to walk in her tenth year. The pubic arch was so narrow that it barely admitted the radial edge of the forefinger. Cæsarian section was deemed necessary. A living child was extracted. Perimetritis and peritonitis

soon followed, and she died twelve hours after the operation. The pelvis measured at the inlet, the conjugate 4'40", the transverse 3'90", and at the outlet the conjugate scarcely admitted of measurement, the transverse gave less than 1'50". The minute investigation of the case led Lange to the following conclusion :—That the pelvis is a kyphotic transversely contracted one, rhachitic and one-sided, with complete ankylosis and fusion of the lumbar vertebræ with the sacrum at one part, with complete ankylosis of the articulating processes of the lumbar vertebræ and of the last two thoracic vertebræ, with complete left-sided ileo-sacral ankylosis, with complete absence of the promontory, with carious destruction of the last sacral vertebræ, with vaulting-over of the inlet by the lumbar portion of the spinal column, as in spondylolisthesis, and several osteophytes in the form of spines.—*Ibid.*

9. Dr. Winkler, of Jena, describes a remarkable *pelvis, which can be dilated or contracted under force*. A girl, æt. 16½, was sent to him to ascertain her fitness for marriage. When a year old she had suffered a pelvic fracture by being run over. She was well built, could carry heavy weights, climb hills, and work with ordinary facility. The dimensions of the pelvis were about normal. It was made out that the ascending ramus and horizontal ramus of the pubic bones of the right side were completely wanting in bony matter, being replaced by a ligamentous substance. This substituted material could be pressed in or out by the fingers. When the legs were stretched apart it became tense, and on bringing the legs together it became flaccid. It was further ascertained that the right sacro-iliac joint admitted of movement.—*Ibid.*

10. Dr. Habit relates a case of dystocia from pelvic tumour. A woman had borne three children alive, without especial difficulty, the last thirteen years ago. For four years she had been aware of a tumour in the hypogastrium. When pregnant the tumour was as big as a child's head. Two distinct tumours could be felt through the abdominal wall, one being the gravid uterus; there was a groove between them. The tumour seemed to spring from the right ilium, pushing the abdominal walls forwards; it was of elastic consistency. By internal examination the pelvic cavity was felt nearly filled with a long, round, firm, elastic tumour, descending from the right sacral angle nearly to the floor of the pelvis. It left at the widest part only 1" space, allowing the finger just to reach the cervix uteri, an inch above the brim. It was concluded that the tumour was a fibroid, to induce labour, and to make attempts to push the tumour out of the way. When labour came on, under chloroform, by the hand in the pelvis, the tumour was raised up, the child's head came down, and it was born alive. The woman did well.—*Wien. med. Ztschr.*, 1871.

11. Dr. Leonard Sedgwick relates two very interesting cases of *fibrous tumour complicating pregnancy and labour*. A lady in her second pregnancy was much prostrated by obstinate vomiting; at the end the abdominal walls were like soft parchment. Towards the middle of the posterior wall of the uterus was a fibrous tumour

as big as a walnut, and in its front were four more, about the same size, firm, without stalk, visibly projecting. She was delivered at term of a healthy boy. She then rapidly recovered. Pregnant a third time, and being very thin, accurate examination was easy. There was no inordinate morning sickness, and no tumours were found. In several successive pregnancies no tumours could be found.

CASE 2.—A lady, æt. 34, had large fibrous growths in the uterus, causing menorrhagia. First pregnancy supervened. When four months gone, after a sudden and violent movement, she had acute pain in the seat of a fibroid. In the sixth month she fell down stairs; considerable hæmorrhage followed. At end of the eighth month the hæmorrhage returned; the left leg and thigh became swollen, hot, and congested; the femoral vein was hard, and apparently blocked up. Labour came on at middle of ninth month. Furious hæmorrhage set in two hours after removal of placenta. A firm fibrous growth was felt protruding from the posterior wall of the uterus. Perchloride of iron stopped the bleeding. She recovered. Shreds of fibrous tissue continued to pass; one piece was as large as a walnut. In three months no trace of enlargement of uterus or of the tumour could be discovered. (These cases are of extreme interest. In the first it would seem that the fibroid tumours disappeared by a process of atrophy; in the second by disintegration and discharge.—R. B.)—*St. Thomas's Hosp. Reports*, 1870.

12. Dr. P. Müller, of Würzburg, relates three additional cases of laceration of the anterior edge of the vulva during labour, entailing dangerous hæmorrhage. He refers to a former series of three cases, one of which was fatal, published in 'Scanzoni's Beiträge' (Band vi).

CASE 1.—Young primipara: head in first position; forceps easily applied. Immediately after release of the head bleeding set in, and continued after the placenta was removed and uterus well contracted. Müller saw the blood streaming from near the clitoris. He tried in vain to control it by pressure and by sutures, and at last plugged. Then two sutures were applied through the labia majora, to keep the plugs in, which also were driven out by coughing. The bleeding was very profuse.

CASE 2.—Primipara, æt. 23; labour spontaneous; perinæum very tense, it split to the anus, in spite of support and lateral incisions. The hæmorrhage was observed immediately after labour, although the uterus was contracted. This was supposed to come from the wounded perinæum, and a bleeding vein was found there and twisted; but active bleeding persisted from a rent directly behind the clitoris, in the depth of which two yawning venous openings were seen. After compression against the symphysis, with perchloride of iron, the two veins with some trouble were seized and tied by two ligatures. The perinæum was stitched with four sutures. On the fourth day it was perfectly united. The wound near the clitoris healed more slowly.

CASE 3.—Primipara, æt. 28. Presentation second cranial. Strong pains drove the head rapidly through the pelvis. Whilst passing through the outlet the midwife reported that the blood ran in a

stream. When seen anæmia was extreme. The uterus was well contracted. A rent was found between clitoris and meatus, from which the blood flowed plentifully. The wound was closed by two stitches, and the bleeding stopped. Death followed in twenty minutes. *Autopsy*.—Structure healthy; uterus firmly contracted; cervix uteri and vagina quite intact; a slight rent in the perinæum. At the fore part of the vulva, close to the clitoris and meatus, was the rent. The bottom of the wound was formed of fine webbed tissue. No large vascular openings were found.

In discussing these cases, Müller says he observed in all that there was increase of bleeding when the uterus was compressed by the hand. This is explained by the fact that the blood is squeezed out of the uterine walls into the connected vaginal plexus.—*Scanzoni's Beiträge*, 1870.

13. Dr. Goodell, in an exhaustive historical and critical review, discusses the various opinions concerning, and methods of, supporting the perinæum. He contends for the following method, as the most rational and useful:—Insert one or two fingers of the left hand into the rectum, the woman lying on her left side, with her knees well drawn up and separated by a pillow, and hook up and pull forward the sphincter ani towards the pubes. The thumb of the same hand is then to be placed on the foetal head, scrupulously avoiding all contact with the fourchette. The right hand assists the thumb in making the head hug the pubes; after a pain it presses back the head from the perinæum, and thus represses reflex uterine action. In this way, Dr. Goodell says, the perinæum is drawn forwards. During a pain the perinæum is tense and inelastic, like the os uteri; in the interval it relaxes and becomes flaccid. Acting on this hint, Dr. Goodell, in cases of rigidity, seizes the moment of greatest relaxation; that is, immediately after a pain, to enucleate the head by hooking the two fingers in the rectum over the chin, and at the same time, by gently sliding back the perinæum over the parietal bosses.—(*Amer. Journ. of Med. Sc.*, 1871.)

14. Dr. Duncan basing on Dr. Lemser's memoir "The Physiological Separation of the Placenta" (Giessen, 1865), calls in question the view expounded by Baudelocque, Schultze, "*the excellent and respectable Ramsbotham*" (who, by the way, was at once the most experienced and the ablest obstetric operator of his day), namely, that the placenta passes through the vagina inverted, so that its foetal surface becomes external. Dr. Duncan says, what Rigby and others suggested, that this inversion is produced by the practice of pulling open the cord. If no interference be practised, but a mark be made on the presenting part of the placenta, to serve for observation after expulsion, it will be seen that the placenta presents by its edge, and in this way is expelled, the mass being folded up longitudinally to pass. To pull upon the cord is to turn the placenta into a sort of sucker, which tends to draw blood from the uterine vessels. He therefore urges the corollary, that in practice the third stage of labour should be left to nature.—(*Edinb. Med. Journ.*, 1871.)

15. A pluripara, æt. 30, in labour at term; on third day occiput presenting, the uterus suddenly burst. Death ensued quickly; gastrotomy was performed in a few minutes; the child was found in the peritoneal cavity, with a large quantity of black blood. A large rent extended along the whole anterior surface; the tissue was soft, degenerated, and emitting an offensive odour. The anterior lip and the tissue at seat of rupture were in encephaloid degeneration.—*Lyon Médical*, 1871.

16. Dr. Hyernaux relates (*Journ. de Méd., &c., Bruxelles*, 1870) two cases in which he performed decollation or bisection at the neck on account of impaction after death of the fœtus from shoulder-presentation, by means of a whip-cord.

17. Dr. Rodenstein refers to a case of Cæsarian section, in which, to control free bleeding, he closed the uterine wound by sutures; the bleeding was stopped. The history of the case is left incomplete; he only further says that "when the operator returned to the bedside he disapproved of the introduction of sutures into the uterus; and to remedy the apprehended evils of this procedure, he opened the abdominal wound on the third day, and removed the sutures from the wound." What was the fate of the patient? The rest of the memoir consists of an historical and critical account of the proceeding. He cites Lauverjat as stating that Lebas de Mouilleron applied the "suture sanglante" to close the uterine wound in a successful Cæsarian section in 1769.—*Amer. Journ. of Obstetrics*, 1871.

18. Dr. Hoschek relates a case in which a gravid woman perishing, he performed Cæsarian section immediately after death. Some little delay occurred through an accident, so that ten minutes elapsed before the child was extracted. It was apparently still, but by mouth to mouth insufflation it recovered.—*Arch. f. Gynäk.*, 1871.

19. Dr. J. W. Roe relates an interesting case of Cæsarian section. Primipara. She had suffered from mollities ossium nearly four years; she had no abortion. Labour occurred (it is presumed) at term. A small, but living and healthy, child was removed; the uterus contracted, and descended into the pelvis. She died four days after the operation. She had been some time in labour; and the pulse had been 120 the night previous to the operation. No attempt at union of the incision was found; the abdominal cavity contained some bloody serum; the intestines were glued together by recent lymph. The uterus was contracted; the edges of the incision made in it were everted, but the cut surfaces were glued together. The pelvis had the typical osteomalacic form. The great contraction at the outlet left no available space for working instruments at the brim.—*Lancet*, 1870.

IV.—PUERPERAL.

1. *Contribution to the Study of Fibrinous Polypi.* By Dr. E. FRÄNKEL.
2. *On the Effect of Spontaneous and Artificial Bleedings.* By Dr. LORAIN.

3. *An Unsuccessful Transfusion.* By Dr. SCHATZ.
4. *Case of Puerperal Tetanus.* By WILLIAM CRAIG.
5. *A Case of Pelvic Cellulitis.* By Dr. HAYDEN.
6. *(Idiopathic?) Gangrene of the Four Extremities, Nose and Ears, following Labour.* By J. R. BEGG.

1. Dr. Fränkel contributes an interesting case to the study of fibrinous polypi. It is headed, "Fibrinous Polypus after Abortion;" five months later death through anæmia, capillary brain-apoplexy, hypertrophy of the heart. The bleedings were very profuse. Being moribund, no attempt to extract the polypus was made. There was found adhering to the hinder wall of the uterus, near the fundus, an outgrowth of bloody villousities. On subjecting this to a stream of water, blood clots fell away, and there remained a mass behind, resembling a villous hæmorrhagic neoplasma. Around it the uterine mucous membrane was thickened.—*Arch. f. Gynäk.*, 1871.

2. Dr. Lorain illustrates the effects of spontaneous and artificial bleedings. In a case of post-partum hæmorrhage the temperature fell to $36^{\circ}8$ Cent. In a case of puerperal convulsions, free venesection after labour, convulsions persisting, albuminuria was practised. Temperature in rectum before bleeding was $38^{\circ}6$, pulse 136. Immediately afterwards the pulse was 160, the temperature had fallen to $38^{\circ}3$. The patient recovered. In another case of puerperal convulsions, with albuminuria, the pulse was 128, temperature in rectum $39^{\circ}6$, that is, 2° above the normal; 1200 grammes of blood was taken; approach to syncope attending. The good effect was immediate, convulsions ceased, consciousness returned. She was delivered a month later of a dead macerated child. The pulse fell to its natural standard in twenty-four hours; the temperature rose at first immediately after the bleeding to $39^{\circ}8$, a slight rise being expected in syncope; it then fell in twenty-four hours to $36^{\circ}6$.

3. Dr. Schatz relates a case of transfusion, which although unsuccessful, is interesting in that the introduction of defibrinated blood into the vein was frequently interrupted by coagulation, a circumstance Schatz thinks due (probably erroneously, R. B.) to the coolness of the temperature of the injection-fluid. Section showed thrombosis and circumscribed phlebitis.—*Arch. f. Gynäkol.*, 1870.

Dr. Craig contributes a case of *puerperal tetanus*. The subject was thirty-seven years old, and had had seven children in Australia. In her eighth pregnancy hæmorrhage occurred twice from a varicose sore in the leg. She was delivered at term by a midwife, and attempts were made by the midwife to bring away the placenta; hæmorrhage ensued, and Dr. Craig was called in, who removed the placenta. The patient rallied, and appeared to be doing well for more than a week. Milk was formed copiously, pulse was 80, it was often 100. On the ninth day she complained of her throat. Difficulty of deglutition increased, and trismus was apparent. Next day she was much worse; the pulse was 130 to 140; opisthotonos was developed; she was perfectly conscious, and described her feeling as if a "ton weight was dragging her head backwards;" the "risus sardonius" was very distinct. She died about forty hours from the begin-

ning of the attack. The case occurred in Edinburgh, in November. The treatment consisted in aperients, fomentations to the throat, Indian hemp, turpentine enema. The fæces had a most offensive smell.—*Edinb. Med. Journ.*, 1870.

Dr. Hayden relates an interesting case of *pelvic cellulitis*. The subject, æt. 30, had been delivered three weeks of her first child when admitted. There was then no hectic, but marked emaciation, great tenderness over the abdomen, constipation. An enema tube could not be made to pass. In the course of a month the abdomen began to swell in a marked degree. The umbilicus projected and gave way, pus being discharged in large quantities. This flowed for a month so freely as to flood the bed. She sank. *Autopsy*.—The exact source of the pus was not determined, but it was considered to be a case of pelvic cellulitis. The uterus was free from disease. There was a great quantity of pus in the abdominal cavity, proceeding from the right iliac fossa. The matter before escaping through the umbilicus had been free in the peritoneal cavity.—*Dub. Quart. Journ. of Med.*, 1871.

6. Dr. Begg relates a remarkable case of *gangrene of the four extremities following labour*. E. R—, æt. 21, was delivered of a healthy boy on the 24th March, 1869, was admitted into the Dundee Infirmary on 25th May. Labour was protracted; nourishment insufficient. She quite recovered from confinement, got about, when, about six weeks after labour, she was seized with an itchy sensation in both her hands, a blue spot appeared on the tip of the nose, hands, and feet, then numbness. She had about sixty-five grains of ergot, in two doses, during labour. She had always suffered from coldness in the extremities, even in summer. On admission, the marks of gangrene were manifest. On the 14th June lines of demarcation were formed. On the 17th, amputation of both legs about the middle was performed. The anterior tibial arteries were found pervious as far as they could be traced. On the 9th July the arms were amputated above the wrists. The patient perfectly recovered. Mr. Begg does not think the ergot given during labour was the cause of the gangrene; there was certainly no ossification of the arteries, no heart disease. He attributes origin to her natural feebleness of circulation in the extremities, aggravated by poor living and the exhaustion following labour.—*Lancet*, 1870.

REPORT ON SCANDINAVIAN MEDICINE.

BY W. D. MOORE, M.D. Dub. et Cantab., M.R.I.A.; and J. W. MOORE, M.D., Ch.M. Dub., Ex-schol. Trin. Coll. Dub.

IN presenting, for the first time in the pages of this Review, in the form of a report, a notice of recent Scandinavian medicine, it may not be amiss to make a few remarks on some points connected with the profession in northern Europe. One of these is the practica

character of its literature. In addition to the many valuable original communications to be met with in the Scandinavian periodicals, the readers of those journals are kept fully *au courant* with the progress of science throughout Europe by means of exhaustive reports. Among the advantages possessed by our Scandinavian brethren of which we cannot as yet boast, are an extensive, although not entirely complete, system of registration of disease, and a liberal co-operation of the State in the advancement of medical science. The former is based both upon the returns furnished by those holding official situations, and upon lists voluntarily sent in by private practitioners. The latter may be exemplified by the munificent contribution of the State, amounting to about 100,000 rix dollars, to the expense of the splendid new pathologico-anatomical institution in Stockholm, and by the travelling stipends awarded from time to time to deserving young medical men to facilitate the pursuit of their studies in the great continental schools. It is interesting also to observe that efforts similar to what are in progress in the several parts of our own country, have been set on foot to establish uniformity of education and reciprocity of privilege, between the three kingdoms of Sweden, Denmark, and Norway. At the meeting of northern medical men held last summer in Göteborg, a committee was appointed to report on this subject to the medical section of the meeting of naturalists to be held in 1873. The promoters of this project expressed their determination to keep it distinct from the question of political Scandinavianism.

The chief medical Scandinavian periodicals are, for Sweden, Norway, Denmark, and Finland: the 'Northern Archives of Medicine;' for Sweden: 'The Hygiea,' a monthly journal of medicine and pharmacy, the Transactions of the Swedish Society of Physicians, and the Transactions of the Medical Society of Upsala; for Norway: the 'Norwegian Magazine of Medical Science,' and the 'Transactions of the Medical Society of Christiania;' for Denmark: the 'Medical Library (Bibliotek for Læger),' the 'Weekly Medical Journal,' the 'Hospital Gazette (Hospitals-Tidende),' and the 'Transactions of the Royal Medical Society of Copenhagen.'

1. The volume of the 'Northern Archives' for 1870 opens with a report, by Professor Abelin, of the children's clinique at the General Orphan House in Stockholm, for the year 1868. Details are here out of the question, but it may be advantageous to endeavour briefly to note the opinions the author has formed upon some interesting points, during twenty-two years' experience in the treatment of the diseases of children.

The General Orphan House is devoted to the reception of destitute children, whose parents or guardians are dead or imprisoned, or are the inmates of hospitals or poorhouses; but other children are admitted on payment in full or in part, by their parents or guardians, of the expense of their maintenance. Among the inmates are to be reckoned also the children of the nurses in the service of the institution. As a rule, no child over six years of age is admissible; but many children who have before been in the house and been

restored to their foster-parents, are, for various reasons, yearly brought back and readmitted, even though they may have passed that age.

The children are divided into the following classes, each class having separate apartments and attendants: 1. Children at the breast, under one year. 2. Children from one to six years. 3. Girls over six years. 4. Boys over six years. 5. Children of classes 2, 3, and 4 when sick. In the dispensary attached to the orphan house, 1935 children were treated during the year. The first part of Professor Abelin's report relates only to children under one year.

The number of these in the house, which, in 1867, was 792, in 1868 fell to 685. Of these 511 were either sent out to nurse, or transferred to another class within the house; 101 died, and 73 remained on the list for 1869. The daily average in the house was only 83.4; in 1866 it had been 153.3. The mortality was 14.7 per cent. The author accounts for this low mortality by the absence of any epidemic, the sufficient number of good nurses, the unusually short average stay of each child in the house, and the good sanitary state of the city at large.

When circumstances rendered it necessary to substitute artificial nourishment for the mother's or nurse's milk, the food employed was unskimmed and boiled cows' milk, mixed according to the age of the child and the nature of its evacuations, with from one twelfth to one sixth of natural Carlsbad water. "Many years' experience," observes the author, "has established the superiority of this to any other nourishment in early infancy, until more varied food can be administered."

Liebig's extract of malt, mixed with egg, milk, wine, &c., has also been employed with varying advantage.

The proportion of good and healthy nurses to the children in the institution is at least 1:1.5. When two children are given to one nurse, one of the former is usually of an age to be partly fed.

Epidemic diseases.—The old experience, that epidemic diseases, especially those most dreaded in childhood, scarlatina, measles, and hooping cough, do not prevail extensively among children at the breast, was this year verified with respect to the last-named disease. The slight tendency to an epidemic which appeared in September, died out spontaneously, without any preventive measures having been taken, after only seven children had been attacked by it. Of these, six recovered: one, a child nearly four months old, died of a complication of the disease with broncho-pneumonia, collapse, and emphysema. The author found bromide of potassium inefficacious in hooping-cough.

Cachexiæ.—On the contrary, prevailed to a large extent. It is true that only three cases of tuberculosis occurred, but rachitis was met with in thirty-two instances, chiefly among children who had been sent healthy, when from three to six months old, to be nursed out of the house, and who had been brought back as sick before the completion of their first year. The author is strongly of opinion that the early and abundant use of amylaceous food is the

most common and the surest cause of rachitis, and there can be no doubt that if such food were made to take the place of milk, which is so admirably adapted by nature to supply the earthy element to the skeleton, the absence of the normal nutriment would greatly tend to produce the disease.

“Among the well known symptoms of rachitis, the laboured respiration merits attention. The mechanism of respiration is altered, becoming strongly abdominal and diaphragmatic, or, in other words, the action of respiration assumes the same character as in the newborn infant. The cause of this disturbed and altered respiration is generally supposed to be collapse of the lungs, the so-called rachitic collapse. That such a condition may occur, and does occur in the cases where the malformation of the thorax is considerable, I will not deny, nor that it is observed after death in rachitic children. But this latter proves nothing, for a rachitic child may, of course, as well as a healthy child, get capillary bronchitis with collapse; but in my opinion we must seek for another explanation of the laboured respiration.”

The causes to which the author refers the disturbed respiration are:—“Partly the greatly swollen and tympanitic abdomen met with in every rachitic child, partly the general relaxation of the muscles. The former exercises a strong pressure upwards on the diaphragm, and the flaccid and weakened respiratory muscles are placed in a condition of visibly laboured action to discharge their functions. In this manner arises the peculiar alteration of the respiratory act, which belongs to rachitis, and which disappears in proportion as the circumference of the abdomen diminishes, and as the muscular power increases, or, in other words, as the rachitis is removed. That these circumstances, if catarrh supervenes, may very easily produce and cause collapse, is evident, but we are not, therefore, justified in asserting that collapse is present in every rachitic child with disturbed and laboured respiration.”

Tuberculosis does not occur very frequently in children at the breast, and its presence is not characterised by any very well-marked symptoms, and is therefore not easily diagnosed with certainty. When it does occur at so early an age, it is seldom confined to one organ, unless perhaps the bronchial glands, but is met with in most of both the thoracic and abdominal viscera.

Among *Intoxications* syphilis alone was met with. Of this all the cases, twenty in number, were congenital. Of the patients, fourteen died; but in all of these, with the exception of two, improvement had taken place, when they were attacked by other diseases, to which the fatal result was attributed. Professor Abelin disputes Dr. Öwre's conclusions as to the etiology of hereditary syphilis, to which we adverted in the 44th volume of this Review, p. 175, and which attributed infection in almost every instance to the mother. Professor Abelin does not go so far in the opposite direction as Zeisel (*‘Jahrbuch für Kinderheilkunde,’* Band i, p. 56); but having had occasion during the last twenty-two years to see and examine in the orphan house at least fifty syphilitic children annually, he has arrived

at the certain conviction that congenital syphilis proceeds as surely and as frequently from the father as from the mother.

The treatment was invariably mercurial. The endermic method was always found preferable to the internal administration of the remedy.

Cerebral diseases.—The author's experience does not confirm the general opinion as to the extreme frequency in early life, at least in their pure and primary forms, of diseases of the brain and nervous system. During the year 1868 only 13 cases occurred in 685 children under one year, and among 1254 cases of illness. Of these 4 were cases of congestio cerebri, 8 of encephalitis and meningitis cerebialis, and 1 of hæmorrhagia meningum.

Diseases of the organs of respiration are for various reasons the most important of the diseases of childhood, and are numerous represented; 287 cases having occurred under eight several forms. Two instances of spasm of the glottis were met with. The author enters at some length into the consideration of the nature of this affection. Some have denied its purely spasmodic character; others look upon the latter as the only conceivable explanation of the suffocative attacks which constitute the essential distinction of the disease. This view was generally received until Kopp described his thymic asthma, since which time at least many German writers have shown a strong tendency entirely to deny the spasmodic nature of the attack. Latterly, however, the former opinion has again found acceptance, especially since the appearance of Friedleben's remarkable work: "*Die Physiologie der Thymusdrüse*," in which the strong statement is to be found, p. 246, "that neither in its normal nor in its hypertrophied condition can the thymus gland give rise to laryngismus; there is no thymic asthma."

Professor Abelin, however, from a careful examination of the tables in Friedleben's book, and from cases which came under his own observation, draws a different conclusion; he says:

"While I thus from my own experience must assume, that a true thymic asthma may be and is met with, I readily admit that suffocative attacks are often produced in childhood by many other causes. There is no practitioner conversant with the diseases of children who has not observed them in rachitis with craniotabes, in whooping cough, in tuberculosis of the bronchial glands, in atelectasis, &c., and sometimes even in cases which in the present state of science must be looked upon as neuroses.

"Another question is, if the proximate cause of the suffocation is always 'spasm,' why not under certain circumstances the opposite of spasm, paralysis or paresis, &c. May not Niemeyer's ingenious theory of the causes of suffocative death in croup find some application also here?" The author thinks that this view is borne out by the result of treatment.

In consequence of overcrowding, draughty ventilation, &c., *bronchitis* occurs more frequently in the orphan house than outside it: 173 children suffered from it during the year. *Capillary* bronchitis, with its ordinary consequences, collapse, bronchopneumonia and

emphysema, constitutes one of the most serious diseases of early infancy. Though most frequently secondary, it sometimes occurs as a primary affection, attacking with extreme violence previously healthy children, and resembling rather an intoxicative than an inflammatory catarrhal disease. Of all the means adopted in this disease, none can be compared for efficacy to the inhalation of watery vapour; so much so that in 1866 a special apartment was prepared to which patients to whom this mode of treatment appears to be applicable can be removed, and where in a separate structure six beds and their occupants can be kept for days or weeks, as may be necessary, in an atmosphere of watery vapour, at a temperature of from 77° to 86° F. The author adds, that these persistent vapour baths, as they may be called, seem to act not only directly on the bronchial mucous membrane, but also on the skin and the secretions in general. Since this mode of treatment has been introduced on a large scale, the mortality from capillary bronchitis has been vastly diminished.

In the second part of this report, Professor Abelin treats chiefly of diseases of the digestive organs, and of erysipelas. The latter disease, as affecting children, he divides into the *puerperal* form, occurring shortly after birth, and the most malignant of all; the *traumatic*, and thirdly, *erysipelas ambulans*. In the last-named variety the author has found the hot bath to be the most efficacious remedy, allaying the pain, diminishing the tension and redness, and apparently exercising a beneficial influence on the general condition of the patient.

In the third number, Professor Abelin gives an account of the working of the *Poliklinik*, or dispensary, attached to the general orphan house, at which 1935 individuals were treated during the year 1868 for 2944 diseases. We shall briefly glance at his statistics.

The cases of epidemic diseases were very few in number, with the exception of those of whooping-cough and ague. Of the former, sixty-one cases (all in children of ten years or under); of the latter, thirty-seven cases came under observation. Of cachectic diseases, rachitis was the most common, occurring in ninety-five instances. The cases of syphilis were thirty-seven congenital and six hereditary. Catarrhal affections of the gastric and intestinal mucous membrane were met with in the surprisingly large number of 1044 cases; a fact attributed by the author to errors in diet. Professor Abelin concludes his report with the details of a case of polyuria, occurring in a child, aged six, and continuing unabated, in spite of the most careful treatment, at the end of a year and a half.

Dr. R. Bergh, of Copenhagen, describes a case of "Pessema, a new form of cutaneous disease." The patient was a prostitute, æt. 43, who had suffered severely from constitutional syphilis. Four days before her admission into hospital an eruption had, without any assignable cause, and with but little constitutional disturbance, appeared on her face, soon presenting some resemblance to variola, with gigantic pustules. The latter were yellow, firm to the touch,

surrounded with a bright red halo. On pressure the investing membrane gave way, whereupon small drops of pus were seen to issue from a mass of little depressions or openings in the soft, spongy surface. Nearly similar pustules, almost as large as peas, were found on the tip of the tongue. The mucous membrane of the mouth was otherwise healthy. Some pustules were met with also in the sternal region and on the backs of the hands. Scabs gradually formed, and slowly fell off. Ten weeks after admission the face was quite free from scabs, but was a little disfigured by numerous red, stellated, slightly depressed cicatrices. On the body and extremities only slight spots, or even no marks, were left. The patient was dismissed in excellent health. Six months later she was admitted into the Kommune-Hospital, where she died in about three weeks of albuminuria (nephritis interstitialis et parenchymatosa, osteitis cranii, pachymeningitis). While she was labouring under the eruption the urine had been examined; its specific gravity was 1.012; it was then free from albumen and from sugar, and contained chlorides in abundance.

Two of the largest elevations removed, as nearly as possible on a level with the skin, were subjected to examination.

"They were void of epidermis. Their sections, washed in distilled water, showed that the tumours were composed throughout of ordinary *granulation tissue*, consisting of small, round, granular cells (identical with pus-cells and white blood-corpuscles), with one or two slightly granular nuclei. The cells lay closely packed together, with a small quantity of amorphous intervening substance, having the reaction of mucin. Numerous capillary vessels ran through the tissue, and made the resemblance to ulcerous granulations complete."

The foregoing case appears to the author to be of the same nature as that of a boy, aged one year, the child of healthy parents, observed by Beigel (Virchow's 'Archiv,' xlvii, 3 and 4, 1869, pp. 367-370, t. xii), and looked upon by him, in conference with Hilton Fagge and Hutchinson, as an undescribed form of skin disease, which he named papilloma area-elevatum. Dr. Bergh proposes to call it *pessema*, from *παισός*, a draughtsman, in consequence of the resemblance of the elevations to backgammon men.

Candidate Thoresen, of Eidsvold, contributes a paper "On the Nature of Contagions, and their Relation to the Human Organism." The author speaks of the several epidemics which have occurred in his practice, in their chronological order.

Diphtheria occurred in 1861, and continued with varying violence as an epidemic for a couple of years. The cases treated were 406, the deaths 93. The author believes the contagion to consist in a matter of organic nature, inimical to the system, which, by contact with mucous membranes, with cutaneous surfaces deprived of their epidermis, and with ulcers, produces, where the poison is not limited, a primary blood-disease, and that it is similar to our other contagious diseases, being most closely allied to scarlatina.

Scarlatina commenced in October, 1866, and the epidemic lasted

until November, 1867. The conclusions which the author believed himself to be justified in drawing with respect to it are:—"1. That the contagion is not any aeriform body, any injurious kind of gas or vapour. 2. That it consists of microscopical bodies, of vegetable or animal origin. 3. That these organisms float about, and are carried up in the atmosphere around the sick. 4. That they attach themselves, and adhere tolerably firmly to other bodies, as clothes. 5. That, consequently, the contagion may be conveyed by healthy individuals. 6. That these organisms are tolerably hardy, and bear severe cold without losing their contagious nature. 7. That the sporadic cases occurring during an epidemic belong to the same epidemic, and are apparently sporadic only because we have not been in a position to discover the connecting link."

Measles commenced in December, 1867, and lasted as an epidemic for six months, in the course of which 271 cases came under treatment. The epidemic was of a benign character, and very few patients died.

Variolæ set in in March, 1868, and lasted about eight months, seventy-six patients having been under the author's care. He believes that vaccination loses its protective power in the course of time, and that, therefore, we find individuals from twenty to fifty years of age to furnish the largest per centage of variolous patients, and the most serious cases.

Professor J. A. Estlander, of Helsingfors, contributes a paper on "le mal perforant du pied," a name which he thinks ought to give way to the more correct term, "leprous ulcer."

Want of space obliges us to pass over many other valuable papers in the volume.

Among the "shorter communications" are:—A case of Traumatic Luxation of the Hip-joint in a child aged five years; replacement after three weeks; recovery—by Professor J. Nicolaysen, of Christiania. Fracture of the Thigh in early childhood, healed with considerable deformity, cured by resection—by J. A. Holmboe, of Bergen. A Contribution to our Knowledge of the Membranes of the Brain and Spinal Cord, with special reference to the serous spaces and lymph-passages, with their connections—by Professor Axel Key and Gustaf Retzius, of Stockholm. A case of Atrophy of the Left Ventricle of the Heart, with Aneurism of the Aorta—by Professor Axel Key, supplemental to his valuable paper on the Reaction of Aortic Aneurisms on the Heart, of which a translation *in extenso* appeared in the 'Medical Times and Gazette' for June, 1870. Reports of Northern Medical Literature for the several quarterly periods are also given.

2. The statistics of morbidity in the city of Upsala are based upon reports sent in by *all* the physicians of Upsala; and although the number of reporters varies from nine to thirteen, it may practically be looked upon as uniform, for the practice of one or more physicians temporarily absent would naturally fall to the lot of some of those remaining in town. We shall briefly call attention to a few points in the remarks prefixed by the author to the elaborate tables in which the reports are utilised.

"During the year Upsala was visited by an epidemic—*measles*. The first case occurred in the beginning of February among some children, eleven days after their return from a visit to a family in Stockholm, in which a child had at the time the premonitory symptoms of the disease. Another of the earlier cases was that of a servant girl, who had gone for two days to Stockholm while the epidemic of measles prevailed there; but, so far as she knew, without her having been in the company of any person labouring under the disease. Twelve days after her return to Upsala the first symptoms appeared. Fourteen days later another girl in the same family sickened, and after another equal interval two little children took ill. The epidemic increased very rapidly, reached its height in the beginning of April, and decreased at about the same rate until the middle of June, when it ceased.

"Of the 947 patients attacked 35 died, 10 with complication of lobular pneumonia or capillary bronchitis, 5 with symptoms of meningitis, œdema, or hæmorrhage in the brain, 1 with diphtheria, and 1 with croup.

"During the winter and spring very many cases of nervous and gastric fevers occurred, though without these diseases having assumed the character of an actual epidemic. During the same seasons a number of cases of varicella were also reported, and in spring in addition several cases of smallpox."

The maximal periods for bronchitis and laryngitis were March and November; the minimal were the summer months. Cynanche tonsillaris followed nearly the same course. The close connection which existed between the frequency of these diseases and the temperature and moisture of the air, is shown in a diagram. The higher the mean temperature of the month and the mean of absolute moisture were, the fewer were the cases of catarrh; and *vice versâ*, the lower the mean temperature, the more frequent was the occurrence of such affections.

In 1869 there was no epidemic of diarrhœa, such as frequently occurs in July and August. On the contrary, the maximums of that disease were met with in February and March, September and November; the minimums, strangely enough, in January and April.

"The close correspondence between the mean pressure of the air and the frequency of diarrhœa is very striking. So soon as the mean height of the barometer rose, the number of cases of diarrhœa diminished, and *vice versâ*. Constant as this agreement was during the year, no general conclusions can, of course, be drawn from one year's observation, as totally different circumstances may possibly have determined the alterations of the disease here referred to."

But every day's experience shows more fully the necessity of accurate meteorological observations in connection with the study of endemic and epidemic disease, facilitated as the comparison of such observations in different parts of the world now is by means of the most wonderful of modern inventions, the electric telegraph.

3, 4. Dr. Larsen presents, in two diagrams, synoptical views of the morbidity of Christiania during the ten years ending 31st of

December, 1869. In one he exhibits the diseases on which the season of the year has an essential influence, as catarrh, rheumatic fever, pneumonia, and diarrhœa; in the other, those considered as infectious, as measles, scarlatina, smallpox, whooping-cough, and mumps.

The greatest number of cases of *catarrh* (737) occurred in February, 1862, which was rather cold, with a high barometer; the disease was least prevalent in the winter of 1862-63, which was unusually mild. In 1864 it was rather prevalent throughout almost the whole year; in June, which was more than usually wet and cold, 354 cases occurred. Rheumatic fever followed much the same course as catarrh.

The end of 1864 and the commencement of 1865 were characterised by the unusual occurrence of a winter epidemic of *diarrhœa*; February having been unusually cold, the mean temperature being as low as 18° F. During 1866 the disease was rather frequent, though not unusually so, which is worthy of note in connection with the prevalence of cholera in other parts of Europe in that year. There was a slight epidemic of cholera in Christiania, but the cases were so few that they are not marked in the diagram. The greatest number of cases of diarrhœa (606) occurred in 1868.

Of the ten summers two were very warm, namely, 1861 and 1868. In the first, which was rather rainy, the cases of diarrhœa were few; in the second, which was dry, they were very numerous, in accordance with the general observation, that dry heat promotes the development of the disease.

Typhus and *ileo-typhus* are, until 1865, not distinguished in the lists sent in. It seems to be pretty well established in Christiania that the greatest number of cases of ileo-typhus are met with in the early winter months. The disease occurred as an epidemic, lasting from the autumn of 1859 to the spring of 1864, and culminating in November, 1861. Since the two diseases have been discriminated in the returns, it would appear that exanthematous typhus is, on the contrary, most prevalent in the spring and summer months.

Epidemics of *measles* occurred in the autumn of 1861, and in the autumn of 1867. In October and November of the former year the number of cases in each month was about 600; in November and December, 1867, the monthly average was between 900 and 1000. The last preceding epidemic of the disease culminated somewhat earlier, namely, in August, 1856.

Two epidemics of *scarlatina* which occurred during the decennial period, also attained their greatest intensity in the colder months.

Smallpox was likewise twice epidemic, namely, in the winter of 1859-60, and part of the spring of the latter year; and from the autumn of 1864 to the end of July, 1865, the number of cases in the latter epidemic having been rather more than four hundred. Since the close of 1866 the cases of the disease have been very few.

5. Professor Stadfeldt's case of *cyphosis dorsalis* was the second instance of cyphosis which had come under that author's notice; the first, an example of cyphosis *sacro-lumbalis*, having been reported in the 'Bibliotek for Læger,' January, 1868. In both instances the

large amount of room in the superior outlet of the pelvis, and the diminution in the transverse diameter of the lower outlet were remarkable. Certain differences, due to the varying situation of the lesion in the two cases, were also observed. In the present instance, a living child, the transverse diameter of whose head was 85 millimetres, passed uninjured between the tubera ischii, the distance between them amounting to only 65 millimetres. This phenomenon the author believes, with Breslau and Hoening, to have its explanation in a certain mobility of the symphysis pubis.

6. The author of the paper last referred to also communicates two cases of parturition with a divided uterus, which had occurred in the Lying-in Institution at Copenhagen, within a year. In the first of these, pregnancy took place in the right half of a partially divided uterus; the labour was natural, but, owing to atony, the placenta had to be removed. It was during the performance of this operation that the malformation was detected. In the second case, pregnancy occurred also in the right portion of a divided uterus, and a cystocele complicated a prolapse of the vagina. There was inertia of the uterus, and delivery was effected by the forceps. The placenta was removed.

Professor Stadfeldt proceeds to make some remarks on vaginal cystocele, and on axal-version of the uterus. With regard to the former, he believes that we must agree with Hecker, who, in considering the etiology of cystocele, attaches most importance to a pathological state of relaxation in the walls of the bladder, and the anterior vaginal wall. Axal-version of the uterus usually consists—though the reverse held in the case given by the author—in the tilting forwards of the left side of that viscus, its right side passing backwards. Freund and Dohrn regard this position of parts as depending upon the pressure exerted on the uterus by the rectum lying to the left, and they explain the axal-version as being in the opposite direction to a “primitive” inclination of the rectum towards the right. This view is not accepted by Professor Stadfeldt, who considers that both the displacement of the intestinal canal and the occurrence of axal-version depend on a tendency to asymmetry. On the existence of this asymmetry in the human skeleton he read a paper before the meeting of Scandinavian naturalists, held in Stockholm in 1863. A translation of this communication appeared in the ‘Dublin Quarterly Journal’ for August, 1864.

7. In the ‘Ugeskrift for Læger,’ Professor Stadfeldt has published a valuable article on the “Induction of Premature Labour in cases of Contracted Pelvis.” The number of the journal in question for November 27th, 1870, contains the conclusions arrived at by the author. While admitting that the operation is often needlessly, or at least injudiciously performed, he yet expresses himself entirely in favour of it under certain conditions, and he illustrates his opinions by detailed accounts of nine cases.

“Premature labour,” he says, “was artificially induced in nine instances; of the children, three died during or shortly after birth, while six left the lying-in hospital in perfect health; and I have

ascertained beyond doubt that of these four were in good health a year subsequently. The mortality was, therefore, 33·3 per cent.; and the result is even more favorable if we regard only the issue of the induction of premature labour in contracted pelvis, with a true conjugate diameter of three to three and a half inches; for of eight children but two died, and the mortality thus amounts to only 25 per cent. On the other hand, as regards spontaneous delivery at full time, thirteen children were born; and as regards premature spontaneous delivery, four children were born. Of the thirteen children only three were living, while ten were stillborn; and of the seventeen children in all, born spontaneously, only four were living, thirteen, on the contrary, stillborn. The mortality, therefore, in the case of the spontaneous births amounts to 76·5 per cent.; and if we carry out the calculation separately in the case of contracted pelvis with a true conjugate diameter of from three to three and a half inches, we find eleven stillbirths in fifteen cases of spontaneous delivery; this gives a mortality of seventy-four per cent., which is again a contrast to the mortality of twenty-five per cent. already mentioned.

“The result for the mother possesses less interest for us, since the numbers are so small in proportion to the mortality assigned to them. It appears to me, however, evident that our clinical experience cannot tell unfavorably in respect to the artificial induction of premature labour. After the spontaneous deliveries, for instance, one mother died; and after the artificially induced labour, none died, if we do not include the sixth case, which, I think, should scarcely be reckoned, as death occurred from meningitis eighteen days after the patient had left the hospital convalescent.”

The professor's rule has been “to resort to the operation with a true conjugate diameter of three to three and a half inches, when former deliveries have shown a disproportion in room, which directly or indirectly has caused danger to mother and child; and this proposition, which has regard to individual circumstances, cannot be subverted by a mass of statistics, but must, according to my opinion, still hold good.”

The greater mortality of boys in cases of contracted pelvis, is next referred to. Of sixteen boys, twelve were stillborn in spontaneous and artificially induced labours together; while of ten girls, six survived. In spontaneous deliveries nine out of ten boys perished.

The author then dwells on the various modes of measuring the capacity of the pelvis which are adopted, and he concludes his paper with remarks on the methods of inducing premature labour. Those recommended are:—an ordinary vaginal douche, in cases where time is but a secondary consideration; the introduction of an elastic bougie into the uterus, combined with the injection of lukewarm water; and that described by Dr. Robert Barnes, namely, the introduction of a hourglass-shaped caoutchouc-bag through the os uteri.

8. In the ‘Proceedings of the Royal Medical Society of Copenhagen,’ for the session 1869-70, are given abstracts of many valuable papers, and also reports of discussions on various important subjects.

Among the latter may be mentioned a debate on the theory of contagion at the meeting of the 14th of November, 1869; and among the former, a paper by Salomonsen on "Aphasia," read at the meeting of October 21st, and a communication by the president on "Purpura Hæmorrhagica" (January 27th). Dr. Salomonsen's article is given at length in an appendix to the "Proceedings."

9. The first of the numbers of the 'Norsk Magazin,' under review, opens with a report on the Medical Department of the Royal Hospital, Copenhagen, in 1869, by Dr. F. Stabell. The statistical returns are illustrated by the notes of numerous cases. A letter on the dependence of the contagion of typhus on external relations follows. "Some scattered Remarks on the System of Military Hospitals," with special reference to the late war, conclude the original communications. The second part of the number is taken up with reports, announcements, and extracts. Among the last is an exhaustive article on "Chloral Hydrate," compiled partly from Liebreich, partly from the 'Medical Times and Gazette.' To the magazine are appended the 'Proceedings of the Norwegian Medical Society in 1870.'

The second number of the 'Norsk Magazin' for 1871 contains a communication on "The Causes of Pneumonia," by Dr. Thoresen, of Eidsvold; and a paper entitled "Remarks on the Position of the Uterus in the Pelvis, and its maintaining powers," by Professor F. C. Faye. Included under the heading: "Reports relating to the State of Medicine in Norway," we meet with letters and recommendations, &c., from the Department of the Interior, which relate chiefly to the proposed introduction of the metrical system of weights and measures into a new edition of the "Norwegian Pharmacopœia," to be published in the course of the present year. It seems that in accordance with a royal decree, the adoption of the metrical system has been actually determined upon, and that after the first of July, 1871, the system in question will in all pharmaceutical and medical processes supersede the old scale of weights and measures. In this respect our Scandinavian brethren are setting us a good example, which we should do well to follow.

From statistics given in the portion of the 'Proceedings of the Medical Society of Christiania in 1871,' which is appended to the present number of the 'Norsk Magazin,' it would appear that variola was scarcely known in that city during 1870. Only fourteen cases of this disease were under treatment throughout the city, in the twelve months ending Dec. 31st, 1870. Of these, *one* occurred in January, *three* in February, *four* in March, *three* in May, and *three* in June. In 1869 there were in all fifteen cases.

10. 'The Upsala Proceedings' contain a series of original papers, together with some translations, read before the Medical Society of that city. Among the former class in the number before us are communications on "Simulation of Unilateral Blindness," by Dr. Björkén; on "Two Cases of Abnormal Situation of the Intestinal Canal," by Robert Murray; on the "Behaviour of the Red Blood Corpuscles with Chloral Hydrate," by Ludwig Djurberg; on the

"Recognition of Opium and Morphia in ordinary Therapeutics," by Joseph Brandberg; "Experiments with Hydrate of Iron and Magnesia as an Antidote," by Oskar Medin; "Experiments with Antidotes for Arsenic," by Ernst Björkman; "On the Passage of Thein into the Urine," by O. Hammarsten; and on "Two Cases of Cercomonas," communicated by P. V. S. Tham. The great length to which this article has already extended prohibits us from entering fully into the consideration of these papers. We would merely remark that Dr. Hammarsten's experiments to determine whether thein passes unaltered into the urine were attended with negative results. The alkaloid in question, when introduced into the urine, was indeed discoverable by the employment of suitable re-agents; but when taken internally, either as thein, or in the form of tea, it appeared only under the guise of an increase in the quantity of urea.

11. Through the three first numbers of the '*Hospitals Tidende*' for 1870, runs a valuable contribution towards the therapeutical history of chloral-hydrate, containing (as the paper does) notes of the use of the drug in various cases of mental disease in the Nørrejydske Lunatic Asylum, by Dr. R. A. Holm, the Physician-Extraordinary. The drug was invariably given internally, and generally simply in solution in distilled water. Three methods of administration, as regards the dose, were adopted. By the first of these, a single large dose (30 to 45 grains) was given in the evening or at bedtime; again, by the second, a large dose was administered, and afterwards smaller doses several times a day; and lastly, it was given in the form of a mixture (2 or 3 to 6 grammes dissolved in 150 grammes of water, the dose being a tablespoonful four times a day). The notes of 20 cases in which the remedy was employed are given at length. Of these 16 were those of patients afflicted with mental diseases, viz.:—8 with *mania*, 2 with *chronic mania*, 1 with *folie circulaire*, 3 with *melancholia*, 1 with *dementia*, 1 with *insanity and strong hallucinations*. One was a case of *epilepsy*, and one of *neuralgia and insomnia*. Besides these, the author conducted a series of experiments on a young medical man in the asylum and on himself, with special reference to the effect of chloral on the temperature of the body. The result of these would go to prove that the thermometer in the axilla usually rises about 4° Fahr. immediately after the ingestion of a full dose of chloral. The article concludes with some general remarks on the character and duration of the sleep induced by the drug.

In the number of the journal for August 17th, G. Lorentzen communicates two cases of poisoning by atropine, in which the Calabar bean treatment proved successful. V. Budde, of the Municipal Hospital (Kommunehospitalet), Copenhagen, writes on the "Quantitative Estimation of Albumen in the Urine by means of the determination of the Specific Gravity before and after the removal of the Albumen by Heat."

Perhaps the most important paper in the present volume is one by Dr. Vald. Rasmussen, of Copenhagen, on the "Operative Treat-

ment of Pleuritis with Effusion." During his period of duty in 1870, as clinical physician in the Municipal Hospital, Copenhagen, he treated 11 cases of acute pleuritis, 3 cases of chronic pleuritis, 1 case of pneumo-pleuritis, 1 case of hæmorrhagic pleuritis, 5 cases of secondary pleuritis (intercurrent with phthisis pulmonalis), and 1 case of pleuritis complicating gastric catarrh. In all these instances the operation of thoracentesis was performed by means of an instrument devised by the author, and suggested to him by Dieulafoy's so-called "aspirateur sous-cutané." Dr. Rasmussen's apparatus is, in principle, a pump-syringe (if we may so term it). The chief advantages possessed by this "capillary thoracentesis" over the ordinary method are briefly the following:—(1) The operation is absolutely free from danger; (2) by means of it the fluid may be completely or almost completely drawn off, and this without the entrance of air into the pleural cavity; (3) it is possible to remove the smallest collections of fluid, whether they exist free in the pleura (in the phrenico-costal fossa) or are encysted; (4) the fluid may be drawn off more slowly or more rapidly, at pleasure; (5) relapses are much less frequent than after ordinary thoracentesis; and (6) the instrument is so small that it does not frighten the patient, and the way of using it is so simple that the operator scarcely requires assistance.

12. The 'Upsala Sanitary Returns for 1870' are the joint compilation of Drs. Bergman and Rubenson, the meteorological data and observations being given by the latter. The year was a healthy one, only 2945 cases having come under the notice of the physicians of the city, against 4340 cases in 1869. The most remarkable diminution was observed in measles, of which, in 1869, not less than 947 cases occurred, while in 1870 only 4 cases were met with. The number of cases of venereal diseases also showed a great falling off. In 1869 456 cases, of which 271 were recent instances of syphilis and soft chancre, and 185 were of gonorrhœa, had been recorded. In 1870, only 288 cases, 140 of syphilis and soft chancre, and 148 of gonorrhœa, came under notice. The author attributes this happy result to the operation of a system of house inspection—a view which seems correct, as the chief falling off was not in cases of gonorrhœa, which would not so easily come under the inspector's observation as would those of true syphilis. This fact should strengthen the hands of the supporters of the Contagious Diseases Acts in our own country.

Meteorological tables of data are appended based on a series of observations taken at the Upsala Observatory *every ten minutes* throughout the year. Five-days' means are also given at length, and an abstract of the leading cosmical phenomena of 1870 is appended.

13. The numbers of the 'Hygiea' last received contain several papers of considerable practical value. Among these are a communication on "Skin-grafting as a Remedy for Chronic Ulcer," by Dr. Axel Lamm, and an article on "Sympathetic Ophthalmia," by Anton Bergh. To each number of the journal is appended a portion of the "Proceedings of the Swedish Society of Physicians, Stockholm."

These contain weekly returns of the morbidity in that city, which are furnished from the various hospitals and charitable institutions, as also from the *private practice* of the physicians generally.

Appendix of journals and papers analysed in the above report:—

1. 'Nordiskt Medicinskt Arkiv.' Redigeradt af Dr. Axel Key. Andra Bandet. Med Tretton Taflor, 1870. Stockholm. Samson och Wallin.
'Northern Archives of Medicine.' Edited by Dr. Axel Key. Second volume. With 13 plates, 8vo, pp. 868.

2. 'Sjuklighets Statistik för Upsala Stad, Aaret 1869,' enligt till Upsala Läkareförening inlemnade Uppgifter. Upprättad af F. A. G. Bergman, Upsala, 1870. Akademiska Boktryckeriet. Ed. Berling.

'Statistics of Morbidity in the City of Upsala for the year 1869,' according to Reports sent in to the Medical Society of Upsala. Arranged by F. A. G. Bergman, Upsala, 1870. Ed. Berling. University Press. 8vo, pp. 7, with numerous tables.

3. 'Tabeller over Folkemængden i Christiania den 31te December, 1869, samt over de i Aaret 1869 indtrufne Dødsfald,' tilligemed en Oversigt over Dødsarsagerne i Tiaaret 1860—69.' Udgivne efter Foranstaltning af Sundhedskommisjonen i Christiania. Christiania. Chr. Schibsted, 1870.

'Tables of the Population in Christiania on the 31st of December, 1869, and of the Deaths in that Year, together with a Review of the Causes of Death in the Decennial Period, 1860—69.' Published under the direction of the Sanitary Committee in Christiania. 4to, pp. 8. With numerous tables.

4. 'Fremstilling af Sygdomsforholdene i Kristiania Decenniet 1860—69.' Af Dr. C. F. Larsen, i Kristiania.

'Sketch of the Diseases of Christiania in the Decennial Period 1860—69.' By Dr. C. F. Larsen. Reprinted from the 'Northern Archives of Medicine.' 8vo, pp. 15. With two diagrams.

5. 'Et Tilfælde af Kyphosis dorsalis med Forsnevring i Tværretning af Bækkenets apertura inferior.' Iagttaget af Prof. Dr. A. Stadfeldt i Kjöbenhavn.

'A Case of Cyphosis Dorsalis, with Transverse Constriction of the Inferior Aperture of the Pelvis.' By Professor A. Stadfeldt of Copenhagen. Reprint. 8vo, pp. 4.

6. 'Et Par Tilfælde af Svangerskab og Fødsel ved Uterus Septus; samt nogle Bemærkninger om Cystocele Vaginale og Axedrejningen af Uterus.' Meddelte af Prof. Dr. A. Stadfeldt i Kjöbenhavn. Med et træsnit.

'Two Cases of Pregnancy and Parturition with Uterus Septus; also some Remarks on Vaginal Cystocele and Axal-version of the Uterus.' Communicated by Prof. A. Stadfeldt of Copenhagen. Reprint. 8vo, pp. 8. With a woodcut.

7. 'Ugeskrift for Læger, 1870, 23—25. Ni Snævre Bækkeners Historie med særligt Hensyn til partus Præmaturus Artificialis.' Af Professor A. Stadfeldt.

'Account of Nine Cases of Contracted Pelvis, with special reference to the Artificial Induction of Labour.' By Prof. A. Stadfeldt.

8. 'Forhandlinger i det kongelige medicinske Selskab i Kjöbenhavn i Aaret 1869—1870.' Kjöbenhavn. F. S. Muhle. 1870.

'Transactions of the Royal Medical Society of Copenhagen for the year 1869—1870.' 8vo, pp. 93.

9. 'Norsk Magazin for Lægevidenskabene.' Udgivet af det medicinske Selskab i Christiania. Tredie Række. 1 Bind. 1, 2, og 3 Hefter. Christiania. Th. Steens. 1871.

'Norwegian Magazine of Medical Science.' Published by the Medical Society of Christiania. Third series. Volume 1. Parts 1, 2, and 3.

10. 'Upsala Läkareförenings Förhandlingar.' Femte Bandet. Åttonde häftet. Upsala, 1870. Akademiska Boktryckeriet. Ed. Berling.

'Transactions of the Medical Society of Upsala.' Volume 5. Part 8. Upsala 1870. University Press. Ed. Berling.

11. 'Hospitals Tidende.' "Optegnelser af praktisk Lægekunst, fra Ind- og Udlandet." 1870. 13de Aargang. Kjöbenhavn. Jacob Lund.

'Hospital Gazette.' "Notes on Practical Medical Science—Home and Foreign." 1870. 13th Annual volume. Copenhagen. Jacob Lund. Folio, pp. 208.

12. 'Sjukligheten och Väderleken i Upsala, året 1870.' A Upsala Läkareförenings Vagnar framställda af F. A. G. Bergman och R. Rubenson. Upsala, 1871. Akademiska Boktryckeriet. Ed. Berling.

'The Morbidity and Meteorology of the year 1870 at Upsala. Compiled for the Medical Society of Upsala by F. A. G. Bergman and R. Rubenson. At the University Press, Upsala, 1871. Ed. Berling. 8vo, pp. 26.

13. 'Hygiea: Medicinsk och Farmaceutisk Manads-skrift.' Trettiondeandra Bandet, November och December, 1870. Trettiondetredje Bandet, Januari och Februari, 1871.

'Hygiea: a Monthly Journal of Medicine and Pharmacy. Thirty-second volume, November and December, 1870. Thirty-third volume, January and February, 1871.

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An Analysis of 140 Cases of Organic Stricture of the Urethra. By John D. Hill, F.R.C.S., &c. London, Churchills. 1871. pp. 59.

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THE
BRITISH AND FOREIGN
MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1871.

Analytical and Critical Reviews.

I.—The Evolution of Species.¹

IN the criticism he has hitherto met with, the illustrious author of 'The Origin of Species' has been peculiarly unfortunate. That just and temperate discussion which is a real help to a man of scientific culture in an attempt to solve a most intricate problem, has been denied him; instead of critics he has met with detractors, and instead of criticism with abuse.

At length, however, Mr. Darwin would seem to have found a critic, we had almost said an adversary, worthy of him. Mr. Mivart's 'Genesis of Species' can scarcely be praised too highly for the candid spirit in which it is written, and for the wide knowledge it displays.

We propose in the following article to examine in detail the arguments of its author, and so by implication those of Mr. Darwin himself, rather than specially to criticise the 'Descent of Man,' the appearance of which happens almost to synchronise with that of Mr. Mivart's work.

Mr. Mivart's own account of his work is that¹

"It is its object to maintain the position that 'Natural Selection' acts, and, indeed, must act; but that still in order that we may be able to account for the production of known kinds of animals and plants, it requires to be supplemented by the action of some other natural law or laws as yet undiscovered. Also that the consequences

¹ 1. *On the Genesis of Species.* By ST. GEORGE MIVART, F.R.S. London, 1871. 1 vol., small 8vo.

2. *The Descent of Man, and Selection in relation to Sex.* By CHARLES DARWIN, M.A., F.R.S., &c. In 2 vols. London, 1871.

² Page 5.

which have been drawn from evolution, whether exclusively Darwinian or not, to the prejudice of religion, by no means follow from it, and are, in fact, illegitimate."

In a note to this, Mr. Mivart points out "that Mr. Darwin himself admits that natural selection has not been the exclusive means of modification, though he still contends that it has been the most important one." Mr. Mivart here and elsewhere fails to see that Mr. Darwin is strictly philosophical in treating only of the action of known causes; and is more likely to lead to the discovery of those at present unknown, by leaving their results as a distinctly marked remainder after subtracting all the results of natural selection, than is Mr. Mivart, by his desultory examination of particular cases, which struck him as difficulties, without the guidance of any theory.

Mr. Mivart, in his introductory chapter, proceeds to say that¹—

"Probably the genesis of species takes place partly, perhaps mainly, through laws which may be most conveniently spoken of as special powers and tendencies existing in each organism, and partly through influences exerted on each by surrounding conditions and agencies, organic and inorganic, terrestrial and cosmical, among which the 'survival of the fittest' plays a certain but subordinate part."

We hardly think that many biologists will be found willing to relegate the subject of evolution almost to the obscurity in which it lay before Mr. Darwin, Mr. Wallace, and Mr. Herbert Spencer threw light upon it by their labours, by attributing all unexplained phenomena to the working of these unformulised² "special powers and tendencies," or content to regard "the whole organic world as arising and going forward in one harmonious development similar to that which displays itself in the growth and action of each separate organism," without attempting to assign reasons for such development; seeing that even in the case of each organism the development is rarely one and harmonious, but a confused series of progressions and retrogressions, such that nothing but the evolution-history of the form seems at all competent to explain it.

Mr. Mivart, at the end of his introductory chapter, brings forward eight allegations against Natural Selection:³

"That 'Natural Selection' is incompetent to account for the incipient stages of useful structures.

"That it does not harmonise with the coexistence of closely similar structures of diverse origin.

¹ P. 20.

² P. 20.

³ P. 21.

"That there are grounds for thinking that specific differences may be developed suddenly instead of gradually.

"That the opinion that species have definite, though very different limits to their variability, is still tenable.

"That certain fossil transitional forms are absent, which might have been expected to be present.

"That some facts of geographical distribution supplement other difficulties.

"That the objection drawn from the physiological difference between 'species' and 'races' still exists unrefuted.

"That there are many remarkable phenomena in organic forms upon which 'Natural Selection' throws no light whatever, but the explanations of which, if they could be attained, might throw light upon specific organization."

The first six of these he proceeds to substantiate in the following chapters; and as the question of what Natural Selection can and cannot explain lies at the root of the matter, we must ask our readers' patience while we examine in detail some of Mr. Mivart's cases, premising that too much weight ought not to be attached to "minute" criticisms of any biological theory, for our present ignorance is such that we cannot as yet hope to discern the actual operation of any law in a vast number of cases of difficulty.

In Chapter II the difficulty of explaining the first appearance of variations of which we can only see the use when variation has progressed to some considerable extent is considered. We have here the old difficulty of the number of grains of corn necessary to constitute a heap, for who shall decide how small a variation may be immediately useful in any particular case?

The first case considered—the neck of the giraffe—scarcely comes under this head, but involves wider considerations, as it shows how great care is necessary in advancing any instance of the working of natural selection in any particular way, and how almost any instance, if not sufficiently considered, may appear open to attack. Mr. Mivart, after stating the usual explanation of its development, namely, that a slightly longer neck gives a great advantage to its owner in seasons of drought, by enabling him to reach higher branches of trees, says that if this were so the necks of some other ungulates ought to have been lengthened likewise. We submit, however, that this does not follow from the premises; for no animal originally less high-reaching than the ancestor of the giraffe could have competed with him; and there is no reason for supposing that any mammal of greater height, with at all similar habits, ever inhabited South

Africa. Mr. Mivart proceeds to quote Mr. Herbert Spencer¹ to show the disadvantage of size to the developing giraffe. In this passage it is pointed out that among animals differing only in size, while the strength to resist incident forces varies as the square of a linear dimension (for it varies as the transverse sections of the bones, muscles, &c.), the forces it is called upon to resist vary as the cube of a linear dimension, that is, as its mass. But the argument of Mr. Spencer is to some extent fallacious, as it does not take account of the fact that the absolute power of work of a muscle varies as its mass, not as its transverse section, and that greater size admits to some extent of a more favorable distribution of matter to resist incident forces.

Again, the external surface from which heat is lost by radiation also varies as the square of a linear dimension, and therefore, although the lung surface from which much of the loss takes place varies more nearly as the mass of the lung, and therefore assuming proportionality as the cube of a linear dimension, the loss of heat, which is the most important form in which energy leaves the body, does not vary nearly as the cube of a linear dimension. Mr. Herbert Spencer mentions this part of the subject in a somewhat unsatisfactory manner, mixing up his mechanical treatment of the subject with the physiological consideration that "organic heat is a concomitant of organic action, and is so abundantly produced during action that the loss of it is then of no consequence; indeed, the loss is often not rapid enough to keep the supply from rising to an inconvenient excess." As a matter of fact, however, organisms are so balanced that it is only the stimulus of unwonted muscular exertion that causes an inconvenient rise of temperature in any animal; also the slightest consideration of the case will show that all work done must be really done at the expense of a corresponding amount of heat that the food taken would otherwise be able to produce. This is an example of the extreme difficulty and danger of the application of deductive reasoning on abstract principles to biological problems, to cope with which properly more extensive data than we at present possess are almost always necessary.

Mr. Mivart next takes up the question of mimicry, and meets with difficulty both in the first beginnings from the mutual neutralisation and obliteration of conflicting tendencies, and also from the extreme minuteness of detail sometimes found, which appears to him unnecessary.

No doubt Mr. Bate and Mr. Wallace place most unwarrantable faith in mimicry, whose value as Darwinic evidence

¹ 'Principles of Biology,' i, 122.

recent observation shows to have been much exaggerated. For the appearance of many striking resemblances in cases in which the habits of the mimic are such that it could not possibly be mistaken for the object mimicked, and the fact that the importance of the enemies against which the mimicry is a protection is often comparatively trifling, seem to show that many mimics have not arisen as such, but are accidental likenesses.

The asymmetry of the Pleuronectidæ is another stumbling-block to Mr. Mivart. Surely the origin of the shifting over of the eye, especially if we allow for the direct action of pressure, is not nearly so obscure as that of the optic nerves in many lower vertebrates; and the remarkable shiftings that nerve-roots undergo during development show that this is merely an extreme manifestation of a common occurrence.

Another difficulty is found in the origin of limbs and their strict limitation to four throughout the Vertebrata. Fortunately we have a sufficiently perfect series of limbs in this class to enable us, with some assistance from the imagination, to trace their development and modification. With regard to their limitation to four, no doubt the presence of a tail, admitting of endless modifications to enable it to be used in every conceivable manner as an aid to locomotion, has rendered the development of additional limbs unnecessary. In this case Mr. Spencer's argument with regard to increase of mass possesses real value, for additional limbs could only be useful at first for some one definite purpose, and if that need were not present would only be an incumbrance to the animal, and add greatly to the expenses of the animal economy.

Again, Mr. Mivart carps at the appearance of the prolonged larynx in the young kangaroo and in the Cetacea, and in no other mammals, whereas, as he says, it could do no harm. A little consideration will show that such a structure would be most injurious when sexual selection or the social instincts of the animal rendered a more highly developed voice desirable, and that in many cases it would interfere with deglutition, and be liable to injury during the process.

The presence of other unexplained structures, and the course of development of some other animals, are also brought up as arguments against "pure Darwinism." On the other hand, it seems to us that Darwinism is the only hypothesis which attempts to grapple satisfactorily with difficulties of this kind, for to attribute the origin of roundabout and uneconomical courses of development, and of elaborate but, from the author's point of view, nearly useless structures, to the action of some higher but unexplained law, seems in the highest degree absurd.

In fact, Darwinism alone explains the coexistence of gigantic universal waste of energy in the inorganic world, accompanied by an economy in some of the details of organized nature far surpassing that found in the working of systems of human design. We cannot help thinking, from the way in which Mr. Mivart states difficult points, that he has in many cases scarcely given them the consideration they deserve. Much that is to him insuperable disappears or becomes greatly lessened before a careful investigation.

Thus, one of his greatest difficulties is the origin of the mammary gland. We have not the advantage of knowing accurately the mammalian pedigree, although Professor Hæckel's labours have greatly lessened our ignorance, and evidence as to the nature of many ancestors can only be obtained from collateral branches; we may, however, fairly assume that the earlier mammals carried their young in more or less perfect pouches.¹

Again, it may be almost necessary for the imperfectly developed young in the pouch to be kept from being dried up by the sebaceous secretion of its walls.

Every step after this towards deriving nourishment, first by absorption through the skin, and then directly through the mouth, would be advantageous. The irritation caused by the contact of the young would stimulate the glands. Finally, the young animal might reach the point of applying its muzzle directly to the skin, as it does in *Echidna*, and would aid by pressure and suction in the conversion under natural selection of the protuberance caused by the ducts of the enlarged glands into a nipple. (In the *Echidna* the development of a nipple may have been rendered unnecessary by the young being held in a fixed position by the walls of the little convolution representing the pouch.) The disappearance of the pouch in proportion as the young are more and more developed *in utero* is a natural step, and is illustrated by the *Thylacine*, which at the birth of its young has a functional pouch; but when the young are able to run alone its enormous dugs hang from a nearly flat surface, bounded by what was the lip of the pouch. The fact that, according to Mr. Lockwood, the young of some fish provided with a marsupium, such as the *Hippocampus*, are nourished by the mucous secretion of its walls in which they

¹ It is noticeable that in the collateral branch, Aves, there are many forms which more or less habitually carry their young. The woodcock conveys its young to feeding grounds between its thighs; many water-fowl carry their young to the ground from their nests, and also allow them to take refuge on their backs; and penguins, which, though highly specialised in some respects, retain certain resemblances to reptiles and monotremes, are said to carry their eggs under the feathers of their abdomen.

are immersed, illustrates by analogy the most difficult step in the course of development above sketched out. To confute Mr. Mivart's argument, it is not necessary to show what the actual course of development has been, but only to point out a way in which it may have taken place.

Mr. Mivart, in arguing that the hood of the Cobra and the rattle of the Rattlesnake are not of use to the animals, and are, therefore, inapplicable in the theory of natural selection, denies the existence of the power of fascination in predatory animals, and claims support from Mr. Bartlett's observations upon snakes in confinement. Such observations, however, are of little value, as a snake in confinement must know that his victim cannot escape, and that he can kill him at leisure; but that some animals capture their prey through the terror which they inspire, no one who has seen a stoat in pursuit of a rabbit can deny. No doubt, however, sexual selection has had the greater share in the production of the organs in question.

The possibility of the improvement by natural selection of an organ like the eye, the perfection of which depends on the harmonious adaptation of so many parts, has been a stumbling-block to Mr. Murphy, Professor Pritchard, and others, besides Mr. Mivart. We can fortunately, however, find eyes in every stage of development, and also of retrogression, and a careful study of them will, we think, answer most objections.

The difficulty of the development side by side of two such superficially similar eyes as those of the cuttlefishes and the vertebrates is only an apparent one, as in the cuttlefish the layer supposed to correspond to the rods and cones of the vertebrate eye lies inside instead of outside the nerve layer, and differs fundamentally in structure. The structure of the lens is also different. In fact, the resemblances are no more than such as seem to be determined by the functional identity of the two organs.

It may be mentioned that among Mollusca the eyes of all the *Cephalophora* agree fundamentally with those of the dibranchiate Cephalopoda, while the eyes of Pecten, and, therefore, probably of other Lamellibranchiates, are more nearly formed on the vertebrate type. Therefore, the divergence in eye-structure agrees with what we know of the divergence of the Cephalophora from an Ascidian-like stock common to them, the Lamellibranches and the Vertebrates.

It is, perhaps, worth mentioning that the eye of the nautilus is utterly aberrant, as, according to Dr. Hensen,¹ no lens exists,

¹ "Ueber das Auge einiger Cephalopoden," 'Zeitschrift für Wiss. Zoo.,' xv, 1865, p. 154.

but the pupil is simply a small hole, the nervous structure agreeing, however, generally with that of the dibranchiate type of eye.

The extreme perfection of the vocal and oral organs in man, and of the former and probably the latter in many animals which make but little use of it, is, no doubt, hard to explain; but without such perfection we should lose, not only æsthetic advantages, but the power of recognising individuals of our own species when out of sight, which must be useful to all animals.

But to criticise as minutely as they deserve all Mr. Mivart's "minute" criticisms would require a volume larger than his, and exhaust the patience of any reader.

The arguments of the writer in the 'North British Review' for June, 1867, on the 'Origin of Species,' some of which are quoted by Mr. Mivart, are sweeping in their scope, and directed rather against the reasoning than the data of Darwinism. The writer first attacks the possibility of indefinite variability, contending that in all cases there exists a limit to variability in any direction. This, no doubt, is true in the main of variation under artificial selection, from which the writer's instances are taken, for most artificial selection is directed only to particular points, and when these have been cultivated up to a certain limit any further development of them would produce monstrosity; but Natural Selection attacks to some extent every part of the organism, and, therefore, tends to produce homogeneous wholes. And we maintain that it would cease to be equally true under Artificial Selection, when the variation is of such a character as to alter considerably the relations of the organism and its environment. Thus, although it is impossible to increase the size of a rose or the swiftness of a race-horse, or diminish the size of a pigeon's head indefinitely, no difficulty has been found in establishing breeds of dogs differing as much in proportions and powers as do the members of the whole family of Carnivora to which they belong. Then follow some objections to the efficiency of natural selection, in which it is attempted to show that, estimated numerically, the value of an advantage derived from a variation is not sufficient to account for the origin of a new species. We will give the argument as it stands.¹

"A million creatures are born; ten thousand survive to produce offspring. One of the million has twice as good a chance as any other of surviving; but the chances are fifty to one against the gifted individual's being one of the hundred survivors (*sic*).

¹ 'North British Review,' June, 1867, p. 288.

No doubt the chances are twice as great against any one other individual, but this does not prevent their being enormously in favour of *some* average individual. However slight the advantage may be, if it is shared by half the individuals produced it will probably be present in at least fifty-one of the survivors, and in a larger proportion of their offspring; but the chances are against the preservation of any one 'sport' in a numerous tribe. The vague use of an imperfectly understood doctrine of chance has led Darwinian supporters, first, to confuse the two cases above distinguished; and secondly, to imagine that a very slight balance in favour of some individual sport must lead to its perpetuation. All that can be said is, that in the above example the favoured sport would be preserved once in fifty times. Let us consider what will be its influence on the main stock when preserved. It will breed and have a progeny of say 100; now, this progeny will, on the whole, be intermediate between the average individual and the sport. The odds in favour of one of this generation of the new breed will be, say $1\frac{1}{2}$ to 1, as compared with the average individual; the odds in their favour will, therefore, be less than that of their parent; but owing to their greater number, the chances are that about $1\frac{1}{2}$ of them would survive. Unless these breed together, a most improbable event, their progeny would again approach the average individual; there would be 150 of them, and their inferiority would be say in the ratio of $1\frac{1}{4}$ to 1; the probability would now be that nearly two of them would survive, and have 200 children, with an eighth superiority. Rather more than two of these would survive; but the superiority would again dwindle, until after a few generations it would no longer be observed, and would count for no more in the struggle for life than any of the hundred trifling advantages which occur in the ordinary organs."

This argument is, no doubt, correct, but it is only partially applicable; for although in the ordinary functions of life the amount of food, &c., obtained by an organism may be proportional to its fitness for the position it occupies, yet some of the most important acts of its existence are of the character of a contest or of a competitive examination, and in all such a very slight superiority gives an organism almost a certainty of success over its competitors, instead of only doubling or trebling its chance of living and breeding. Periods of scarcity or disease give rise to such competitions, and sexual selection owes its great power to its acting in this manner. It must also be remembered that all these arguments based upon probabilities have precisely the same weight when applied to the question of the origin of permanent varieties as to that of the origin of species,

and we have observed and know that varieties do appear; therefore they do not affect Mr. Darwin's attempt to prove that the origin of species is the same in kind with that of varieties.

The reviewer goes on to state very clearly the bearing of the principle of the dissipation of energy in the time allowable for the production of species. The complexity and abstruseness of this subject renders any lengthened treatment of it here out of place. We can only remind our readers of the extreme difficulty of all calculations in cosmical physics, and the great uncertainty of numerical results based on data so little known as those which determine the rate of loss of the sun's heat and the retardation of the earth's rotation. Even the very causes that retard the earth must convert its *vis viva* into heat, and thereby prolong its habitability. Some of Sir William Thomson's arguments are open to the gravest objections; for instance, it does not follow that, because the form of the earth is that due to its present velocity of rotation, therefore it must have solidified when it had a rate of rotation not much differing from the present, if, as many physicists suppose, the earth must be regarded as viscid under the action of such forces as determine its shape, and, therefore, as having its shape altered as these forces alter. Similarly, calculations as to the rate of loss of the sun's heat must, until some one theory of the constitution of the sun meets with general acceptance, be regarded with the greatest suspicion, however high may be the authority of their author.

Thus, although Sir William Thomson leaves a very great interval between the limits which he sets to the time at which the earth became habitable, no one can receive these limits as absolutely determined. The geological data for determining the age of the earth give, if possible, still less definite results, but on the whole point to a time far earlier than Sir William Thomson's earliest limit; they can at best only give us the latest possible time at which the earth can have become habitable, for any amount of destruction and reconstruction may have taken place before the earliest extant rocks were formed. Again, as pointed out by the reviewer, from the dissipation constantly taking place, the energy of physical forces must have been greater at an early period than at present, and therefore the rate of physical change must have been greater, but by how much we cannot tell. But the most ancient deposits, not too much metamorphosed to supply means of judging, accord precisely in their physical constitution with those of many recent formations, and from similarity of effect we may in general infer similarity of operating forces.

Therefore the time of their formation may very probably have been long since the first habitability of the globe.

But this greater rapidity of physical change at an early epoch implies at least an equal increase in the rate of organic change, for fewer forms will be in that comparatively fixed state which, according to Mr. Darwin, is probably the condition of the majority at any one time, while the changing action of the environment, both directly and by conducing to migrations, will be constantly stimulating the origin of and giving permanence to new varieties.

And even were it not for this, the number of varieties that occur is so vastly in excess of the number which find places to occupy in the economy of nature, that there is little danger of any new place being made by physical change without there being found some variety ready to take advantage of it.

Attempts to compare numerically the amounts of organic differentiation required to produce varieties, species, genera, and larger groups, are at present hopeless, and but little weight can be attached to such a passage as the following :

“We doubt whether a thousand times more change than we have any reason to believe has taken place in wild animals in historic times would produce a cat from a dog, or either from a common ancestor. If this be so, how preposterously inadequate are a few hundred times this unit for the action of the Darwinian theory.”¹

In answer to this we may urge generally, that the classification of the organic world has furnished Mr. Darwin with his strongest arguments. From its being impossible to frame any accurate definition of the amount of difference necessary to constitute a variety, a species, or a genus, and from these groups being connected together like the branches of a tree, he argues the existence of a real affinity of descent. To this the North British Reviewer and others have replied that, if any limits to the possible variety of form of organisms in a group exist, as all the organisms of the group must be between them, if only they be sufficiently numerous, some of them must necessarily be very much alike, and that, therefore, confusion would exist whatever was the origin of the forms. This is illustrated by comparison with the classification of inorganic, natural, and, artificial products, the classification of which is often a matter of the highest difficulty, notwithstanding that in these cases we can generally analyse much more accurately the causes of affinity and difference. We can only say that we can trace no appearance of this close packing of forms between

¹ ‘North British Review,’ June, 1867, p. 301.

limits in nature, and, as the arrangement of organic nature is certainly not linear, we cannot conceive its practically occurring. There is no analogy between the classification of organic and inorganic bodies, except in cases in which the latter owe their origin to the working of the human mind, for the difficulty of classifying inorganic bodies is not so much in drawing lines between groups as in allotting individual objects, which often partake of the characters of several widely different classes, to their correct groups.

In the case of the products of the human brain, however, in the fine and the mechanical arts, a real analogy does exist between natural and artificial productions, for the productions of almost all the arts in universal use have been developed into their present forms almost precisely as the Darwinian hypothesis requires organisms to have been; the only radical difference being due to the fact that the human mind has always a share in the multiplication of individuals, and stands in the same relation to the artificial world that life, whatever it may be, does to the natural.

No better instances can be found of the operation of the principles of Darwinism than those brought to light by the labours of modern philologists;¹ the difficulties of philology are precisely those which Darwinism explains in the organic world, but, fortunately for philologists, the development-history of very many languages is pretty well known. If we consider a language as a whole, we may observe its early variation from its parent stock, and can often trace that what appear to be chance variations are due to the environment, that is, to the constitution of the race speaking it; but still more often observe that variations, which at present are so utterly inexplicable that they can only be attributed to chance, are preserved for their convenience or agreeableness;—reasons which correspond to the causes of natural and sexual selection. In nature we cannot at present altogether explain the appearance of variation; but neither Mr. Darwin nor any other evolutionist—except, possibly, the dummy “pure Darwinist” set up by Mr. Mivart only to be knocked down—would conceive that such effects could be manifested without corresponding causes. And the occurrence of parallel geographical varieties of different species—for instance, the occurrence of sets of forms in a particular district, all differing in the same manner from the corresponding forms in another district, as in the case of the twenty-nine kinds of American trees, all differing from their nearest European allies in a similar

¹ See the late Prof. A. Schleicher's work, translated by Dr. Bickers, ‘Darwinism tested by the Science of Language,’ 1869.

manner, quoted by Mr. Mivart from 'Proc. Acad. Nat. Sci. Phil.' (vol. i, 28, 62), or of the butterflies, which have tailed races in some places and tailless in others, or large wings in some localities and smaller, differently shaped ones in others—may ultimately guide us to those causes.

In each particular case special causes are at work, but the law of the survival of the fittest holds throughout, and a close investigation shows that a great number of the special features of organic development are admirably displayed in the arts.

Dress, for instance, in which a desire to conform to custom takes the place to a great extent of heredity, and checks indefinite variation, affords curious instances of rudimentary organs.

The buttons above the tails of our coats now only cause a trifling annoyance by catching in chairs, not sufficient to cause rapid extinction, and will, we hope, never again support our sword-belts. Our coat-collars and cuffs, our evening coats, and innumerable parts of academical, professional, and servants' clothes, whose variations are checked by exceptional restraining causes, remain as rudiments of structures that came into existence, either directly from utility, or from the action of the environment in selecting chance modifications. Dress also shows how structures may continue long after they have become, not only useless, but even injurious in a slight degree, for probably there can be no one who has thought on the subject who could not modify his dress so as to make it more comfortable, if custom were not too strong for him.

Sir John Lubbock has shown what sufferings savages undergo under the influence of custom and sexual selection for the sake of personal adornment.

Dress also, to some extent, justifies Mr. Darwin in attaching more importance to Natural Selection operating on what, from ignorance, we must call chance variations, than the direct action of the environment, for most temporary failures arise in this way. In the purely practical arts, however, variation hardly ever takes place without obvious and sufficient reason.

Painting affords an instance of the rapid development of improved forms followed by great specific fixity; sculpture, of "rudimentary organs" in mock antique costume.

In architecture rudimentary organs are very common; useless gurgoyles project beyond modern gutters: and in many modern timber roofs conspicuous but useless wooden trenails merely serve to conceal the iron bolts used in their construction; this may be considered the *reductio ad absurdum* of rudimentary organs. Even in the mechanical arts such instances are common; thus, railway carriages for a long time retained many characters inherited from chariots and coaches, and are

only now learning to be built without them. In shipping such rudiments are still more marked. Take, for instance, the painted port-holes, which, with sham wooden guns, originated in an attempt to intimidate hostile vessels and prevent their approach, but which could not, as they are now painted, deceive any one.

Perhaps, until our knowledge of organic nature is greater, as much evidence in favour of the working of Darwinic principles may be obtained from the study of the arts as of nature, and such evidence, although only collateral and not direct, is already strong enough to prove that Mr. Darwin's are "*veræ causæ*," their sufficiency remaining to be established by a consideration of the organic world with the help of the light thrown upon the working of the laws by a study of the simpler cases.

Mr. Mivart considers that Mr. Darwin is wrong in disbelieving altogether in sudden considerable variations being perpetuated in nature, and quotes many believers in evolution in support of his views; but that he can adduce a certain number of cases requiring the preservation of such sudden steps for their explanation, and also several instances of such jumps occurring under domestication, by no means shows that they are not exceptional, and the fact that they have attracted so much attention is rather a proof of their abnormality.

The question of the infertility of distinct species when crossed is so familiar that little remains to be said on the subject. Mr. Mivart states this difficulty in Darwinism, which is, no doubt, one of the greatest, without throwing any new light on the subject. Mr. Darwin has, in previous works, attempted the explanation of all the points raised by Mr. Mivart, with at least partial success.

It appears that, since it is manifestly impossible that widely different organisms should breed together, as heredity would entail incompatible structures on the offspring, therefore fertility must cease at some point. On the other hand, it is found that the greatest fertility does not result from the pairing of the most similar forms; for such pairing brings out, not only the excellences, but the defects of both, and these defects may require but little development to render them fatal. It seems probable that the evils of "in-and-in breeding" are wholly due to this.

From what has been said it follows that, between the limiting cases of absolute similarity and greatest difference between the parents, there must be some point at which fertility reaches a maximum; this maximum seems usually to occur when the differences between the parents is sufficient to constitute variety, and that after this it falls off, at first gradually, until we come

to that point at which the line between species and variety is usually drawn. After this the degree of fertility falls off very rapidly, but not really suddenly, and absolute infertility can only be said to be reached when even the first stages of development are not commenced after the access of the male; and even here there is no very distinct break, for some of the earlier stages seem often to occur without impregnation at all, and so sexual reproduction merges into parthenogenesis.

The fact that to a superficial observer the question of fertility between two organisms admits only of a positive or negative solution, whereas there exists really every gradation between fertility and infertility, tends to throw a false light over this subject, as does also the fact that many definitions of species draw the line between species and variety by means of the mutual fertility test, so that there is a tendency to argue in a circle; it is an almost absurd criticism upon this that, practically, the line is not drawn at the point of mutual infertility, but at that at which the product of a cross is infertile.

Mr. Mivart, after going through the ordinary objections based on palæontology, which have been sufficiently fully considered by Mr. Darwin, dwells more at length on the question of the origin of certain well-marked aberrant groups, such as the Pterodactyles, the Aves, the Ichthyosaurs and Plesiosaurs, the Whales, and the Tortoises.

Considering from what slender sources the pathological knowledge we have of Aves and Pterodactyles is derived, it can be no cause of wonder that the hiatus between them both and ordinary reptiles remains so wide. With the larger marine reptiles and cetacea his case is stronger; the number of specimens preserved is very great, and their larger bones are so indestructible that it might be thought that almost every individual of the larger forms that reached maturity would leave some remains; and we cannot suppose that the intermediate forms inhabited districts whose geology is unexplored; this difficulty, therefore, must be regarded as, at present, inexplicable. It may, however, be pointed out that the theory of sudden considerable variation fails equally, for Mr. Mivart cannot suppose that such gaps as those between Ichthyosauria and Plesiosauria, or between other mammals and whales, can have been bridged over by a single sudden step.

Mr. Mivart himself does not appear to attach much weight, except as cumulative evidence, to certain exceptions to the laws of geographical distribution, which laws may be considered as the very foundation of Darwinism. The very fact that the exceptions which he points out seem so very remarkable is the strongest confirmation of the universality of the laws in ques-

tion, and Mr. Darwin has pointed out, in the 'Origin of Species,' many ways in which exceptional cases may have arisen.

The author then proceeds to discuss the origin of serial, lateral, and vertical homologies, and rejects, as insufficient, Mr. H. Spencer's explanations, that the serial homology of Annulata is due to their being tertiary aggregates, that is, genetically, chains of individuals formed by generature, which, instead of separating, have remained coalesced as a single individual, while the homologies of the spine and limbs of vertebrates are due to similarity of condition. He says (p. 154), "But there are, it is here contended, abundant reasons for thinking that the predominant agent in the production of the homologies of the limbs is an *internal force or tendency*" (the italics are his). Having called in this *Deus ex machina*, the whole difficulty is solved; he may as well do all as a part of the work; the author, therefore, endeavours to make out that as much as possible remains unexplained by Mr. Spencer's theory, in order that we may feel the need of him more strongly. Mr. Spencer's theory is certainly presented to us in a highly abstract form, and is really little more than a statement with regard to organic nature of the principle, the general truth of which no one will deny, that the present state of any body is the immediate result of all the forces that have ever acted upon it, directly or indirectly, and that it therefore contains within itself all the essentials of its history, if we could only read them aright; and he would be the last to pretend that enough was known of the way in which heredity has handed on modifications produced by incident forces, sometimes preserving structures to which the forces at present in action could never have given origin, at others allowing incident forces to sweep away the hereditary insignia by which the kinship of the organism was best marked, to enable us to give anything approaching to an explanation of each particular case.

Yet few will be prepared to leave this fairly safe ground for Mr. Mivart's internal force theory, of which, however, we have no opportunity of judging, as he tells us nothing as to how this force is supposed by him to act; he merely adduces a series of cases of homology and symmetrical abnormalities of structure, which, he says, are explained by no other theory, and which necessitate the hypothesis of an internal law or substantial form, and whose difficulties, together with all those of ordinary development, and also of evolution, vanish, according to him, upon the acceptance of the doctrine of some such law, which shall account, at the same time, for specific divergence as well as for specific identity.

We can only compare this part of our author's argument to that common among all uninformed people who are wont to explain

every phenomenon which they do not understand by attributing it to "electricity," and who seem to think, because they are told on good authority that many things they do not understand are due to electricity, that "what I do not understand" and electricity are convertible terms.]

Mr. Darwin's theory of pangenesis, if freed from some of the restrictions with which its author surrounds it in his first enunciation of it, is sufficient to explain much for which Mr. Mivart requires his "internal force." If it once be granted that only representative gemmules need be present in the fertilised ovum, and that gemmules from homologous parts are mutually replaceable within certain limits, the correlation of the variations of homologous parts, the tendency to the repetition of similar parts and symmetrical diseases, would be accounted for. It must be conceded to Mr. Mivart that similarity of incident forces is not a sufficient explanation.

It is extremely unfortunate that Mr. Mivart's chapter on evolution and ethics appeared before 'The Descent of Man,' for many of his objections are answered in it, although the answers must have been written before seeing the objections.

Mr. Mivart contemplates civilised morality, and compares it with what may be called the morality of animals, and very naturally can trace little or no connection between them. He finds no such thing as "formal" morality among animals—no sign, for instance, of moral reprobation. No doubt, for formal morality could not exist until the human mind had so far progressed as to be capable of forming abstract ideas, notwithstanding that the moral sense on which it rests may have considerably advanced in the absence of that power of mind.

The author also objects that some very common moral acts are neither for the benefit of the individual nor of the society—for instance, the tending of the aged and infirm. This is an extension of the principle of doing good to others of the same community, and one that would almost necessarily follow when the moral sense of the community had become somewhat developed, although it is inconsistent with the causes that originated the principle itself. Many parallel cases may be pointed out in which the instincts of animals of obvious general utility lead them to act in certain special cases against their own interest. Mr. Mivart's difficulty as to the perception of degrees in morality is well explained by Mr. Darwin in his 'Descent of Man,' where he attributes it to the more permanent sense of satisfaction resulting from the higher line of conduct, as compared with the fleeting satisfaction given by the gratification of temporary appetites and emotions.

Mr. Mivart questions with great justice many of Mr. Herbert

Spencer's views, and amongst others his opinion that for every immoral act, word, or thought, each man during this life receives minute and exact retribution. If arguments against this doctrine from history and experience were not forthcoming, we conceive that the principle of natural selection would furnish one almost unanswerable, for surely, if the bad were so heavily handicapped, the morality of the human race could never have made a progress so very halting and interrupted.

Mr. Mivart next criticises with great severity and much justice the theory of pangenesis, which, from the extreme difficulty of applying to it any crucial test, will probably long remain *sub judice*. For the benefit of some of our readers, we will give Mr. Darwin's latest enunciation of it ('Descent of Man,' vol. i, p. 280):

"According to this hypothesis, every unit or cell of the body throws off gemmules or undeveloped atoms, which are transmitted to the offspring of both sexes, and are multiplied by self-division. They may remain undeveloped during the early years of life or during successive generations; their development into units or cells, like those from which they were derived, depending upon their affinity for, and union with, other units or cells previously developed in the due order of growth."

Mr. Mivart seems to think that Mr. Darwin considers the gemmules themselves to be the ultimate atoms of the body, whereas he distinctly states that he considers their product—the cell—to be so, and on this ground objects that they cannot be allowed to be capable of self-division. Mr. Darwin probably does not suppose this self-division to take place except when the gemmules are so far developing as to be taking up nutriment, for he illustrates his theory by a comparison between the increase of the germs of cholera and rinderpest and that of his gemmules in general. If he did not do this few physiologists would be inclined to accept his theory.

Mr. Mivart produces cases which he thinks incompatible; that, for instance, of mutilation. This has occurred for a vast number of generations, in the form of circumcision, the continued necessity for which seems, at first, difficult to explain on this hypothesis. Mr. Darwin, in his 'Variation under Domestication,' vol. ii, p. 23, quotes from Blumenbach ('Philosophical Magazine,' vol. iv, 1799, p. 5) the statement that, in Germany, Jews are often born in a state such as to have given origin to the term "born circumcised," and also alleges that any mutilation, to be hereditary, according to this theory, ought to take place at the time of the first development of the organ mutilated, because, during its existence previous to mutilation, its cells have

the opportunity of giving rise to sufficient gemmules to preserve it for future generations, at least up to that stage of development at which mutilation takes place. He explains cases of the inheritance of mutilations by the hypothesis that they occur when the wound caused has remained long enough to have presented its sloughing surface, and thus to destroy all the gemmules belonging to the lost part. Certain breeds of pigs, which are born with tails, almost invariably lose them by disease at a very early age. This would seem to be a case in point.

The strong and increasing body of physiologists who have ceased to regard a cell as the physiological element, but consider it as the product under the action of the environment of an elementary portion of germinal matter, probably agree largely with Mr. Darwin in substance, although the form of his enunciation would require considerable modification to bring it in accordance with their views. It seems merely necessary to add to their statement of observed facts his theory of the almost ubiquitous distribution within the body of minute particles of the germinal matter of each homogeneous organ which we know to undergo self-division, and which may do so to a far greater degree than we suppose, to have nearly the pangeneses theory; and, perhaps, if we do not bind these gemmules always to reproduce themselves exactly—which, indeed, is inconceivable, for then no variations in kind, such as the appearance of an extra finger, would be possible—but leave them free to be developed in many different positions where tissue of apparently precisely identical structure occurs—having, indeed, a strong tendency towards precise reproduction of their parent structure, but admitting of modification by their environment—we may explain the various facts of homology of parts in an individual. No doubt this modification of the theory sacrifices much of its sharpness, but it is contended that without some such relaxation the appearance of new additional abnormal structures is inexplicable.

It is impossible in a short space to give a fair idea of Mr. Darwin's case in favour of pangeneses, and we must refer our readers to his work ('Animals and Plants under Domestication') for it; but it must be granted that it fits in with and explains a vast number of phenomena; and although it is necessary to concede to these gemmules the possession of a large number of properties difficult to reconcile with their supposed simplicity, it would seem that the same difficulties are really involved in all other theories of reproduction, although they do not appear so conspicuously in such as are less analytic.

Mr. Galton's recent experiments on the transfusion of blood in rabbits must be held to be indecisive, from their limited

number and from the probability that only the gemmules of the blood itself are present in it at any one time in any number; extensive experiments on grafting are, perhaps, more promising.

Perhaps the connection between the generative and the urinary system may stand in relation with the probable fact that gemmules would be likely to escape from the body in company with the products of the waste of the tissues in which they originated.

Mr. Mivart professes in his eleventh chapter to give his views on specific genesis by means of the internal force or tendency of which he speaks; we cannot say that we have succeeded in grasping his meaning satisfactorily. No doubt there exist internal properties and tendencies in different organisms on which the environment can act; for anything without properties is inconceivable; but that innate tendencies without any secondary cause should give rise to new species seems but a return to the Special Creation hypothesis in disguise. The author says that his views agree to a great extent with the Derivation hypothesis of Professor Owen; this may well be, for both seem equally without a *vera causa* to rest upon, and although Mr. Mivart spares us the mysterious declamation in which those of the Professor are shrouded, his real views are nearly as hard to make out.

Of the action of Natural Selection we now know a good deal, but little compared with what remains to be known; of that of the environment generally we know little or nothing, as a consideration of geographical varieties, and of the action of drugs, will show; and until we have some means of knowing how far these two causes are capable of explaining the origin of species, there is no need to go in search of purely hypothetical causes of change.

The great length to which our remarks on Mr. Mivart's most interesting book have extended leave us but little space to treat of the still more important work of Mr. Darwin; this is of less consequence, as by far the greater part of it consists of facts illustrating the subject treated, and a mere summary of these and the deductions from them is unnecessary, as the author, with his usual consideration for the weakness of the human memory, has supplied in the body of his work both special and general summaries. He has also in general so carefully verified his facts that they are safe from all attack, but in the present volume we cannot but feel some suspicion of a few of the anecdotes of animals quoted from Brehm's 'Thierleben,' and also some of the old statements about ants made by P. Huber, than whom, however, no higher authority exists. We will confine ourselves

to a few remarks on some of the more important deductions from these facts.

After showing that man's mental attributes differ in degree, not in kind, from those of the lower animals, and that we share with them almost equally many instincts and instinctive habits, Mr. Darwin comes upon the difficult ground of the origin of the belief in God. He says, vol. i, p. 65—

“There is no evidence that man was aboriginally endowed with the ennobling belief in the existence of an Omnipotent God. On the contrary there is ample evidence, derived not from hasty travellers, but from men who have long resided with savages, that numerous races have existed and still exist, who have no idea of one or more gods, and who have no words in their languages to express such an idea. The question is, of course, wholly distinct from that higher one, whether there exists a Creator and Ruler of the universe; and this has been answered in the affirmative by the highest intellects that have ever lived.”

Now, we venture to maintain that the questions are not wholly distinct—that is, whether this Creator and Ruler of the universe is a concrete God, who can be worshipped and prayed to, and not the shadowy Unknowable, the truth which he whom Mr. Darwin speaks of as “our great philosopher, Herbert Spencer,” finds hid at the bottom of a corrupt mass of religious beliefs as our “Ultimate Religious Idea.” Now, we believe that not only an overwhelming numerical majority among civilised men, but an equally powerful intellectual one, would decline altogether to regard this as a religious belief at all. It is a necessity of thought, no doubt, and, as such, important. Mr. Darwin says (p. 68)—

“The feeling of religious devotion is a highly complex one, consisting of love, complete submission to an exalted and mysterious superior, a strong sense of dependence, fear, reverence, gratitude, hope for the future, and perhaps other elements.”

Now, we venture to say that no intelligent man could look upon the Unknowable with any of the feelings here specified. Fear of the unknown is, indeed, justifiable, for it may become known and injurious; but fear of the unknowable would be worse than childishness. Possibly, amongst the doubtful “other elements” may be included a certain vague awe, inspired by anything stupendous, but not really akin to fear, which is, indeed, common to the God of civilised mankind at large and the God of Mr. Spencer, but only because the former includes the latter.

How, then, are we to reconcile Religion and Darwinism? No task is more difficult than that of bridging over the gap between

Religion based on Truth, and Deduction and Science based on Observation and Induction, and we wish that task were in abler hands, as, until it is to some extent accomplished, we fear that many evolutionists have their religious beliefs shaken, and many religious people dread Darwinism, both, we believe, most unnecessarily. The vital importance of the matter must be our excuse for a few remarks, of the imperfection of which we are deeply conscious. For brevity's sake we must refer our readers to works on the special subject for the support of our assertions.

First. We maintain that man has a soul, and not a mere belief on the part of the majority that he has one.

Secondly. That a soul does not admit of being developed like a mental quality.

Thirdly. That animals have no souls.

Therefore, we conclude that, at some time or other, when man became really man, he had a soul given him, and that this process may be called the creation of man. At what period in man's development this took place we do not venture to suggest; possibly not until he had become a tool-using, talking animal. Possibly the savages spoken of in the passage above may still be without one. In making this assertion we, of course, contradict Mr. Spencer's explanation in the 'Fortnightly,' May 1st, 1870, p. 555, "that man was led through dreams, &c., to look upon himself as a double essence of spiritual and corporeal," while granting that the idea of the distinction between body and mind has been developed. Nor would we be supposed to deny that religious ideas, both true and false, have undergone development. But we do assert that the existence of a soul, involving a belief in a future state and of the Deity implanting it, and rendering a revelation possible, cannot be due to development; and to those who deny the existence of a soul and a future state our argument has absolutely no weight. We cannot see that a belief in this one supernatural intervention need diminish the willingness of any man to receive Mr. Darwin's explanation of the origin of the Moral Sense in the succeeding chapter, in which we can detect no flaw; and we cannot but admire the wonderful fairness and judgment displayed in deducing the difference between right and wrong from the greater persistence of the instinctive tendency to action in the former case.

But little remains to be added to his treatment of the subject. In urging arguments in favour of the heredity of virtue the case is even understated. The subject is complicated by the difficulty of estimating the effect of education, which seems to be very great, for we often find that the families of men whose eminent excel-

lence has brought them into a position in which they have but little time for their domestic duties are often much below the average in moral goodness.

The most important question of the probable effect of civilised man having, to a great extent, released himself from the action of natural selection is treated of at some length by Mr. Darwin. That this is an element of serious danger to modern nations cannot be doubted; and as far as we can see, our best hope lies in the springing-up of the idea of the immorality of those who are in any way physically or mentally unsound marrying, for we fear no legislative enactments will control men in this matter. The spread of self-respect and education will rapidly diminish the numbers of those degraded classes which multiply with great rapidity from the absence of all prudential restraint after marriage, and the feeling that nothing can further impoverish them.

Of the remainder of Mr. Darwin's account of the principal difficulties in tracing the connection of man with the lower animals we have no space to treat, neither can we examine the enormous collection of facts bearing on sexual selection, which, with a most careful investigation of the laws which appear to be deducible from these facts, occupy two thirds of Mr. Darwin's work. We confess to a slight feeling of disappointment when first we found how small a portion of the book was taken up by matter bearing directly on the development of man, but this feeling soon passed away when we discovered the extreme interest of the part on sexual selection and its important indirect bearing on the main question. The excellence and lucidity of the descriptions, and the remarkable merit of the woodcuts, especially those which are original, do much to heighten the pleasure of reading it. Those who are familiar with Mr. Darwin's writings will be much struck by his increased self-reliance, and by the many indications that he feels himself entitled to take many things for granted which formerly he would have thought it necessary to prove. In this he is fully justified by the marvellously rapid change which has taken place of late years in the view generally taken of his theory.

We have observed above that the philosophical way of attacking the subject of the origin of species is by considering what may be accounted for by the known *veræ causæ* of the action of the environment and of natural selection, and then endeavouring to find an explanation for the remaining phenomena, if any. It appears to us that a set of phenomena is already separated out which, although due to the action of the environment, yet involve special questions and deserve consideration apart.

It is manifest that the liability to being variously modified by

drugs and new and unusual conditions of climate cannot have been developed in organisms by natural selection; it is what Mr. Mivart would call innate, that is, it is due to the various physico-chemical properties of the components of the organism.¹

It is a very general feature in the effect of these agents that they at first give a great shock to the organism, which, if it survives, is followed by an organic change often very obvious, though only understood in a very few cases, possibly due to the replacement of certain chemical compounds in the organism by substitutive products—metamers, isomers, &c.—and after this change has taken place the organism is but little influenced by the continued operation of the agent, and even suffers if it ceases, but is able to live under circumstances that would prove fatal to a number of the same species that had been through this change. Now, in different geographical districts different agents must operate on organisms. We know how great are the changes produced on man by waters containing minute quantities of certain substances and by small changes of diet or of climate. Similarly Mr. Darwin mentions that a diet of fish or the acrid secretion of toads will cause changes in the plumage of parrots, and it is commonly known that a diet of hemp-seed will turn a bullfinch nearly black. Now, the species or geographical varieties of bullfinch inhabiting the Palearctic region must be submitted to differences of diet, &c., far greater than that between a diet of rape- and canary-seed and a diet of hemp-seed: will not this account for their differences, which are but little more important than that between an ordinary and a blackened bullfinch?²

It seems highly probable that the sudden appearance of the black-shouldered variety of pea-fowl, which Mr. Selater considers a distinct species, independently in several flocks of pea-fowl in England, may be explained in this way, as well as other analogous instances, some of which we have mentioned above.³

We even venture to suggest that mimicry may be sometimes due to these causes; for instance, the same causes may have determined the general coloration of both the mimicked and the mimicking butterfly, and the minuter likenesses alone may be due to Natural Selection.

Similarly, animals mimicking plants are exposed to the same

¹ See 'Descent of Man,' pp. 151, 152.

² In connection with this must be considered the facts of correlation, such as the deafness of white cats with blue eyes, and the liability of animals of certain colours to certain poisons. See 'Animals and Plants under Domestication,' vol. ii, ch. xxi, for many instances.

³ Page 269.

physical and even to some extent the same chemical conditions as the parts of the plants they mimic; the colouring matter in both is often identical, or nearly so, and often depends to a great extent upon the light to which it is subjected. For instance, spiders that are in the habit of hiding in the axils of leaves must be subjected to the same conditions of pressure, light, and air, as the buds themselves, and these conditions may produce likeness enough for Natural Selection to operate on.

It is found that the presence of minute quantities of certain substances in saturated solutions will determine the form in which a salt crystallizes, and other similar facts are known which by analogy elucidate this subject. Such are the phenomena of allotropism.

Enough has, we think, been said to show that nothing is at present likely to throw more light on the origin of species than a careful investigation of the effects of chemical and physical agencies on an organism slightly different from those under which it usually lives, and this ought to be followed up by an analysis of the circumstances under which geographical varieties exist, and an endeavour to find the cause of their differences.

II.—Diseases of Women.¹

PROBABLY no branch of the medical art has made more important progress within the last twenty years than that generally comprehended under the title of "diseases of women."

Long a neglected, if not a despised department of medicine, it lay, as it were, dormant for years, and it was only after the invention of exact methods of physical examination, such as the speculum and the uterine sound, that its advance towards the prominent position it now holds commenced. Greatly as the speculum has been abused by those ignorant of its uses, it is certain that the services it has rendered to this department of the profession can hardly be over-estimated. Indeed, it is

¹ 1. *The Diagnosis, Pathology, and Treatment of Diseases of Women.* By GRAILY HEWITT, M.D., F.R.C.P., Professor of Midwifery, University College, London.

2. *Traité Pratique des Maladies de l'Utérus et des ses Annexes.* Par A. COURTY, Professeur de Clinique à la faculté de Médecine de Montpellier. Paris.

3. *A Practical Treatise on the Diseases of Women.* By T. GAILLARD THOMAS, M.D., Professor of Obstetrics, &c., in the College of Physicians, New York. Philadelphia.

4. *A Practical Treatise on the Diseases of the Sexual Organs of Women.* By F. W. VON SCANZONI, Professor of Midwifery in the University of Wurzburg. Translated by A. K. GARDNER, A.M., M.D. New York.

not too much to say that Récamier's invention has done for diseases of the uterus what Lacnec's discovery has done for diseases of the chest. In the one case, as in the other, the physician had of old to trust to general symptoms, apt to mislead. No better illustration of this could be given than the fact mentioned by Dr. Henry Bennett in his classical work on uterine inflammation (a book which, from the existence of certain unfortunate prejudices at the time of its appearance, has never yet been estimated at its proper value), that Sir Charles Mansfield Clarke, in his work on diseases of women, published so lately as the year 1831, does not even mention the existence of inflammatory erosion and abrasion of the cervix uteri; and yet it is not too much to say that in this condition we find the key to a vast number of cases of uterine disease. In making these remarks we do not at all mean to say that all, or even much, of this improvement is due to the use of the speculum itself, but rather that the exact methods of physical examination, by which alone we can arrive at an accurate estimate of any morbid state of the uterine organs, coincide with the introduction of that instrument into general use.

Much of the increased attention paid to female disease in this country is unquestionably due to the untiring energy with which the late Sir James Simpson worked at the subject, and to the fact that his pupils carried his doctrines far and wide; and British obstetricians are justified in reflecting with some pride on the large share which their labours have had in aiding the progress of gynæcology within the last quarter of a century. It is impossible, in the short space of one article, to attempt anything like a comprehensive discussion of recent advances in the knowledge of female disease; all that we now propose is to consider briefly a few topics of importance, and by taking for our text the four standard works we have selected for review we shall be able to contrast the views of prominent teachers in England, France, Germany, and America. All these works are held in high estimation in their respective countries, and may be considered as probably the best and most recent exponents of the doctrines held in them.

Dr. Graily Hewitt's volume is doubtless well known to all our readers, as it has met with much well-deserved success. Dr. Hewitt has adopted a cumbrous and, as we venture to think, an injudicious method of treating his theme, which impairs the usefulness of the volume. It is an annoying thing to have to refer to half a dozen different places before one gets a complete description of any disease, the symptoms being in one place, the treatment in another, and so on. But, in spite of this drawback, the work is the result of much careful study and

research, and has certainly increased the well-earned reputation of its author.

Dr. Courty's large volume is one of the most elaborate and complete treatises on diseases of women in existence. Although it has the fault of unnecessary diffuseness so common in all French medical writings, it is unquestionably a work of the utmost value.

America has of late years been celebrated for the amount of work done on this subject, and Dr. Thomas's volume is the latest and, we think, one of the best books on diseases of women published in that country. It is a carefully-considered and talented volume, up to the latest information, and especially good as regards the treatment recommended.

Of Scanzoni's treatise but little need be said. The high estimation in which it is held in Germany is, perhaps, the best proof of its value, and Dr. Gardner's excellent translation puts it within reach of all who are unfortunate enough not to be able to read it in the original.

As we have alluded to the use of the speculum, we cannot do better than commence by the consideration of those cases in which it has rendered most service.

"What's in a name," is a saying that may most justly be quoted in reference to those uterine lesions in the detection, but still more in the treatment, of which the speculum finds its most useful application. The absence of a definite nomenclature has, indeed, been the cause of infinite wrangling and misunderstanding on this point. Whether we choose to call the phenomena to be detected through the speculum, and which previous to its use were altogether unrecognised, by the name of ulceration, erosion, abrasion, villous hypertrophy, granular inflammation, or any other of the many aliases by which the subject has been obscured, is a matter of comparatively little moment. Of their immense importance, however, there is now among educated physicians no two opinions. It is, indeed, a curious chapter in the history of medicine to look back upon the fierce and acrimonious discussions which twenty years ago raged on this matter, and which must have seriously retarded the progress of gynecology. Fortunately these conditions are not now looked upon as being of themselves morbid alterations of primary consequence, but as the signs of more deep-seated changes which cannot otherwise be recognised, an idea well expressed in the words of Scanzoni:—"This form of ulceration never exists alone; it is always associated with other profound alterations of the tissue of the uterus, most frequently engorgement or chronic catarrh, which must sometimes be regarded as the cause, sometimes the consequence, of the ulceration." These words express,

as we believe, the true pathology of this condition, and this view is evidently more or less shared by all the authors under review. Thus, Courty tells us that "a granular state of the cervix cannot continue long without inducing engorgement and, more often, hypertrophy of the cervix uteri, and, what is more serious, inflammation of that or of the whole of the uterus." While Courty thus recognises the important fact that granular inflammation is generally attended by more deep-seated changes, it seems to us that he is wrong in looking upon it as the cause of the associated changes.

We believe that extensive granular disease can be traced to the existence of a morbid state of the structures beyond the os, and that it is, as a rule, a secondary and not a primary condition. This is evidenced, not only by the profuse glairy discharge which is seen oozing out of the os uteri, and which is necessarily secreted by the mucous membrane lining the cervix and body of the uterus, but by the fact that in such cases the os uteri is always abnormally patulous, so that the sound enters freely, and that the cavity of the uterus is found to be lengthened. Dr. Thomas more correctly talks of the granular condition of the cervix "as one of the elements of endometritis; but it assumes such peculiar forms, and becomes of itself so absorbing a subject in a therapeutic point of view, that it appears necessary to treat of it apart." It is with reference to treatment that a true view of this condition is of so much importance. As long as physicians continue to look upon granular inflammation of the cervix as a strictly local condition, unassociated with more or less deep-seated alterations, they will content themselves with occasional applications to the exterior of the cervix, which may produce a temporary alleviation of the symptoms, but which are utterly powerless to cure the severer forms of uterine catarrh, which depend, probably, on similar changes in the mucous membrane beyond the os uteri. Of the intractability of the worst cases of this kind we have abundant evidence, and the utter uselessness of the usual treatment is candidly confessed by Scanzoni, who thoroughly recognises the importance and insidious character of the affection. As the opinion of so high an authority cannot but carry weight on a subject the importance of which is not yet sufficiently recognised in this country, we may be excused for quoting his remarks.

"We may esteem ourselves fortunate if we can moderate somewhat the hypersecretion of the uterine mucous membrane, and moderate its consequences. As for ourselves, we do not remember a single case where we have been able completely to cure an abundant uterine leucorrhœa of several years' standing. Many women whom we have been called upon to treat had to attribute to a

neglect of this disease a bodily and mental debility which they would keep for the rest of their days, or hysterical attacks which deprived them of all enjoyment of life."

This is a picture drawn in sombre colours, but the faithfulness of which no one who has seen much of uterine disease will be inclined to dispute. As long as the disease is not attacked at its seat, it is not surprising that the treatment should be uniformly unsuccessful. To treat a morbid state of the mucous membrane lining the interior of the uterus by applying nitrate of silver, or tincture of iodine to the exterior of the cervix, is about as likely to be as useful as would be a similar application to the exterior of the throat in affections of the pharynx or tonsils. That the desired results can be most satisfactorily obtained by properly chosen methods of intra-uterine medication is the universal experience of all who have practised this method of treatment. Amongst this number Dr. Hewitt would appear not to be classed, as he seems doubtful whether any benefit is to be derived from intra-uterine medication. Both Courty and Thomas have devoted much attention to the point, and their observations will well repay perusal. The limited space at our disposal will not admit of a full consideration of their views, which we can only allude to in a very cursory manner.

Both of them have tried the injection of fluids into the uterine cavity, which naturally suggests itself as the simplest means of making the necessary applications. So many disagreeable, and even serious, consequences have, however, followed the use of these remedies, that they both unhesitatingly reject them. These do not seem to result from the agents used, but from the retention of the fluid in the uterine cavity, and the severe uterine colics induced by the efforts of the uterus to expel its contents. It seems essential, indeed, to dilate the cervix artificially before they can be safely used, and this is too tedious and painful a procedure to be resorted to as frequently as is necessary in such cases. Courty's favorite plan, which he assures us he has adopted with perfect success, is to introduce a piece of lunar caustic into the cavity of the uterus, which melts there and runs over the whole lining membrane. That the plan is beneficial we willingly admit on the assurance of so reliable an authority, but the prolonged contact of the alterative application seems to us to be likely to be occasionally hurtful. Thomas recommends the introduction of various ointments by means of an instrument devised by him for the purpose. Other and, as we believe, more easily applied and more effectual means of arriving at the same results have been recently described in this country, from the use of which we have

attained the most beneficial results. Into the details of this method of treatment, however, we cannot enter, our object being simply to assert the principle that morbid conditions of the lining membrane of the uterus can only be effectually treated by direct applications, and to bring forward two such high authorities in favour of the safety and efficacy of the plan. At the same time, it is evident that intra-uterine medication in rash and inexperienced hands is a double-edged tool, which might well give rise to much mischief. This is, of course, no argument against its use in suitable cases, but a word of caution may not be unadvisable. Courty's observations on this point are very valuable, and should be carefully borne in mind before resorting to any such treatment. He says—

“The first and most important contra-indication is the existence of a marked inflammatory state of the uterine system. Such treatment should never be employed when there exists a marked metritis, a peri-metritis, or ovaritis. Attention to this rule will prevent many accidents.”

In this view we cordially concur.

By carefully selecting the cases, and by due attention to these precautions, many cases which have resisted all other treatment may be speedily and effectually cured, and it is to be regretted that so valuable a method of treatment should not be more widely known and better appreciated in this country than it now is.

Uterine deviations have now for many years been a favorite *casus belli* between different schools of gynæcologists. Sir James Simpson worked at the subject with his usual vigour and energy, and his pupils, imbibing, as they could not fail to do, some of his marvellous enthusiasm, have spread his doctrines far and wide. The presence of flexions could be readily enough made out; the treatment by mechanical means was something tangible and striking. The various pessaries which filled the surgical instrument-makers' shops, the intra-uterine stems, the Hodges' and their infinite variations, exercised the ingenuity of physicians, each of whom attached his name to some petty modification of the original pattern, and then claimed to be an inventor. Then came the fierce discussion which raged some years ago as to the safety of such methods of treatment, especially in the Academy of Medicine of Paris, in which Depaul and Valleix held the chief parts, and in which, as is usually the case in such controversies, both the safety and danger of mechanical contrivances were greatly exaggerated. All these things served to make the topic a favorite one for discussion. Of late years, however, more rational

and less one-sided views have been gaining ground, and those who have watched the controversy must admit that, as so often happens, neither side can claim with justice a complete victory. The balance of opinion, however, has been decidedly in favour of that view which looks at a flexion as not in itself so much a disease as a symptom, a theory that cannot be better expressed than in the words of Scanzoni, that "flexions of the womb do not acquire any importance, nor are followed by any serious danger, save when they are complicated with an alteration in the texture of the organ." Of our authors, three decidedly hold this view, of which Scanzoni is, indeed, the leading exponent. Dr. Graily Hewitt, however, as is well known, is a stout and zealous champion of the opposite side. In his eyes flexions of the womb are the fruitful parents of most of the ills which female flesh is heir to. He seems to find them everywhere and in all circumstances, even as a late paper read before the Obstetrical Society shows, in pregnancy, where he thinks they cause the sickness which so constantly accompanies that state; and as he has invented a variety of formidable looking instruments expressly to correct the misplacements which he believes to be so common, it may be worth while to consider the evidence on the subject, believing, as we honestly do, that the free use of mechanical contrivances in hands less able than his own may lead to grievous mischief. This is all the more necessary, as Dr. Hewitt's book enjoys so wide a reputation, and as the treatment he recommends is likely to be extensively followed.

Now, in carefully reading the symptoms that authorities in uterine disease ascribe to flexions, it cannot fail to strike us that they are precisely the same as those met with in engorgements and congestions of the uterus unaccompanied by any displacements, such as pain, bearing down, inability to walk, and the like. On vaginal examination, also, the same local phenomena are recognised in both, such as enlargement, tenderness on pressure, and evidence of increased vascularity, the only difference being that in the one case we have, in addition, the signs due to displacement. These facts are admitted by all. Then, again, the same causes produce the like symptoms, whether displacement attends them or not, and in labour and miscarriage, in defective involution, in frequent childbearing, we have the usual antecedents of the symptoms referred to. That these are the most common causes of flexions even the most ardent advocates of the mechanical theory seem to admit. Thus, Dr. Hewitt says, "We most frequently find, however, that the increased size of the uterus consequent on gestation is the forerunner of flexions, especially of retroflexions." Then cases are not wanting, and must have been frequent in the experience of all

who see much of uterine disease, in which well-marked flexions exist without any symptoms whatever, and which are only discovered quite accidentally when, for some reason or other, a vaginal examination is made. How often this condition exists without being detected it is, of course, impossible to say, but we may safely assume that the discovery of flexions which are unaccompanied by symptoms must be the exception rather than the rule. Now, it must be admitted that engorgement and congestion of the uterus is precisely the condition which we should *à priori* expect to be the cause of displacement, from the hypertrophy and over-weighting of the uterus which accompany them.

We arrive, then, at these results :

1. That engorgements and chronic inflammations of the womb produce certain marked symptoms where no flexions exist.

2. That flexions exist, accompanied by similar engorgements, with precisely the same symptoms.

3. That flexions exist, with no evidence of accompanying engorgement, in which none of the above-mentioned symptoms are met with.

It is scarcely surprising, therefore, that, in the face of such evidence, teachers of the eminence of Thomas, Courty, and Scanzoni should refuse to give to flexions any but a secondary place. The question is by no means devoid of practical importance, for they, and those who agree with them, set to work to remedy the "*fons et origo mali*" with rest, leeches, sedatives, and other appropriate remedies, using mechanical contrivances as adjuvants only ; while Dr. Hewitt and his followers immediately introduce their pessaries, trusting chiefly or only to them, to the exclusion of other methods of treatment. They, of course, point to their results, and assure us that nothing is more common than to cure a flexion by a pessary alone.

Scanzoni has something to say on this subject which is not without interest. He tells us that his predecessor at Wurzburg, the celebrated Kiwich, was a great believer in mechanical treatment, and that he has already carefully examined no less than twelve of his patients who had been thus treated and were supposed to have been cured, and in whom afterwards the flexions were found to exist ; and Courty tells the same tale of patients supposed to have been cured by Valleix.

It is unquestionable, however, that great benefit and relief are often obtained from the application of a properly adjusted support. This is admitted by all ; only it is maintained, and we think rightly, that pessaries and uterine supports should take a secondary and subordinate position, that they should be looked upon as adjuvants only, and that our main reliance should be placed in remedies calculated to remove the causes on which the dis-

placements depend, and, above all, that they should not be used when there is evidence of acute congestion or much tenderness. To fix a uterus in such a state in a cradle pessary may possibly be a harmless means of treatment under Dr. Hewitt's supervision, but would, as we know for a fact that it occasionally has done, prove a very serious thing for the patient in other hands. The same remarks apply, of course, with greater force to the varieties of intra-uterine stem pessaries at one time much used by Simpson. But on this it is hardly necessary to dwell, because, though they may still find some theoretical supporters, we are happy to think that they are now but rarely employed, and in very exceptional cases only.

Few departments of gynæcology have attracted more attention of late years than the various inflammations which are of such common occurrence in the neighbourhood of the uterus, and the variety of nomenclature which has been adopted with regard to them has done much to increase the prevalent confusion on the subject. Pelvic abscess, pelvic cellulitis, peri-uterine engorgement, pelvi-peritonitis, peri-uterine inflammation, para-metritis and peri-metritis, are some only of the many names by which writers have attempted to describe these affections, and to enforce their own views of their pathology, and it is scarcely surprising that they have managed to make the subject obscure and perplexing to the student. Its importance can, however, hardly be over-estimated, and the greater the experience gained in female disease the more convinced does one become of the frequent occurrence of the class of complaints alluded to, and of the necessity of having distinct views of their nature and diagnosis. Their history, too, is not altogether flattering to the modern gynæcologist. It is certain that the disease was familiar to the ancients, who have left a description of it, which, although it may not be so accurate as modern writers would pen, still proves that they knew at least more about it than any practitioner living only twenty years ago. It was not until the writings of Marchal de Calvi, Nonat, Simpson, and a few others, about the fourth decade of the present century, again directed the attention of the profession to it, that it began to be seriously studied. Since that time a mass of information has been acquired on the subject; but no writers have done so much in advancing an accurate study of the varieties of pelvic inflammation as MM. Bernutz and Goupil, in their now classical treatise, published in 1857, and this, perhaps, chiefly because of the somewhat narrow and one-sided view they so ably upheld, which referred the vast majority of cases of the so-called peri-uterine cellulitis to localised peritonitis, and assumed that inflammation

of the cellular tissue around the uterus did not exist as a pathological reality.

A statement so sweeping naturally caused much discussion, and discussion and study can do nothing but good. The result has been a much more careful inquiry into those affections, and considerable progress towards a recognition of their varieties and differential diagnosis. Of the authors under review, Thomas is the one who, it seems to us, gives much the most clear and trustworthy description of the subject. Courty dwells on it at great length, and his views, on the whole, coincide with those of Thomas, but his description is somewhat diffuse and difficult to follow. Hewitt mentions the views of Bernutz and Goupil, but seems inclined to adopt the more prevalent opinion of the extra-peritoneal seats of inflammation and makes no sufficient attempt to differentiate the classes of cases. Scanzoni is nearly altogether silent on the topic. Thomas's chapters on pelvi-peritonitis and pelvic cellulitis are amongst the best in his work, and place the subject in a clearer and, as we think, in a more accurate light than any other writer we know of, excepting, perhaps, Matthews Duncan, who has published a separate work on 'Parametritis and Peri-metritis,' in which they are discussed with much fulness and detail. We cannot enter into the *questio vexata* of the exact seat of the disease, but must cordially endorse the opinions of Courty and Thomas, that both varieties of inflammation exist, and are, as a rule, capable of distinction.

To all anxious to master a complicated question we can recommend a study of Thomas's carefully written chapter, but we must be pardoned for extracting the table in which he attempts to sum up the signs by which a differential diagnosis may be arrived at.

Peri-uterine cellulitis.

1. Tumour easily reached, generally felt in broad ligaments, and may be felt above pelvic brim.
2. Marked tendency to suppuration.
3. Abdominal tenderness, chiefly over the iliac fossæ.
4. Tumefaction generally noticed laterally in the pelvis.
5. No constitutional signs of peritonitis present.
6. Tendency to monthly relapses not marked.
7. Retraction of thigh not rare.
8. Pain severe and steady.
9. Face is not much altered.
10. Nausea and vomiting not excessive.

Pelvic peritonitis.

1. Tumour very high, only in vaginal *cul-de-sac*; does not extend above superior strait.
2. Suppuration rare.
3. Abdominal tenderness excessive above brim of the pelvis.
4. Generally noticed near or upon median line.
5. Constitutional signs of peritonitis present.
6. Tendency to relapse every month very marked.
7. Retraction of thigh never occurs.
8. Pain excessive and often paroxysmal.
9. Face is very anxious.
10. Nausea and vomiting often excessive.

Peri-uterine cellulitis.

11. Does not necessarily displace uterus.
12. Not accompanied by tympanites.
13. Uterus fixed to a limited extent.

Pelvic peritonitis.

11. Always displaces uterus.
12. Always accompanied by tympanites.
13. Uterus immovable on all sides.

This table may be considered too precise to be reliable, and it must be admitted that many of the points are very open to question; still, it is valuable as a general guide, and may prove of assistance to some of our readers in the decision of a difficult question.

The secondary results of these peri-uterine inflammations, long after all actual inflammation has passed away, are not by any means worked out, and are, we feel convinced, of considerable importance. No writer that we know of has discussed this topic at any length, with the exception of Dr. Duncan in his work already referred to, and yet fixation of the uterus, the result of adhesions from past inflammation, is very commonly met with indeed. When once attention has been drawn to this point, the gynæcologist will almost daily meet with cases in which they exist, and in which they suffice to explain symptoms otherwise very obscure. This topic is certainly well worthy of much more study than it has received, chiefly, no doubt, from a diagnostic point of view, as little that we can do has any power in hastening a cure. Still, a knowledge of the existent condition, and of the probability that time will eventually effect its removal, is of itself of the utmost importance, and may save us much doubt and the patient much unnecessary medication.

Allied in many respects to pelvi-peritonitis, and not infrequently mistaken for it in its less severe forms, is the disease now generally called "pelvic hæmatocele," and which, like pelvi-peritonitis, was practically unknown to gynæcologists only a few years ago. Although few of the diseases of women have been made the subject of more study, still there are many points in connection with its pathology and treatment which are still undecided, and which will well repay further investigation. Dr. Graily Hewitt's chapter on this subject is amongst the most carefully written and most valuable in his work. It contains in a concise form the most recent information on the subject, and is well worthy of careful study. Dr. Thomas's and Dr. Courty's writings on this subject are also good and reliable, but Scanzoni has, to our surprise, passed it over with a very brief and imperfect notice, by no means worthy of so eminent an author. Although the disease is certainly not a very common one, still it is sufficiently so to be not infrequently met with by all who see much of female disease. It is strange, therefore, to find a physician of Scanzoni's experience stating

that he has only once met with an hæmatic effusion of any size, except in the puerperal state, and that he must, therefore, borrow from others all that he has to say on the symptomatology, course, and treatment of the disease. It is not easy to account for so strange a statement, which is certainly at variance with the experience of most obstetricians. The two points on which there is still most difference of opinion are—as to the precise seat of the hæmatic effusion, whether intra- or extra-peritoneal, and as to the advisability of active interference by puncture of the tumour. With regard to the former point, Courty adopts, without hesitation, the views so ably advocated by Bernutz and Goupil, and held by the majority of French pathologists, that the hæmorrhage is almost invariably intra-peritoneal. Without absolutely denying the possibility of hæmorrhage taking place externally to that membrane, he considers it to be so excessively rare, and so little dangerous, as to justify him in practically leaving it out of consideration. This is surely a very one-sided view of the question, and we believe Dr. Hewitt to be much nearer the truth in admitting the not infrequent occurrence of the extra-peritoneal variety of hæmorrhage. It is true that this has been but rarely proved by post-mortem examination, while there is abundant pathological evidence of the existence of the intra-peritoneal variety of the disease. The rarity with which extra-peritoneal hæmorrhage is found after death has, however, been well shown by Matthews Duncan to depend on its being comparatively less formidable than when blood is poured into the peritoneal cavity. Physically, it is hardly possible to distinguish them, but there seems to be abundant clinical evidence, short of post-mortem examination, of extra-peritoneal hæmorrhage. In this view we are pleased to find that Thomas concurs. With regard to treatment, the balance of opinion has, of late years, been tending to an expectant method, not interfering by puncture or otherwise unless signs of softening or inflammation render it essential. This point, however, is by no means satisfactorily settled, and in a recent paper read before the Obstetrical Society Dr. Meadows has brought forward some evidence in favour of more frequent operative interference, which is certainly worthy of careful consideration, and calls for a more extended inquiry into this important branch of the subject.

There is, probably, no department of gynecology in which within the last quarter of a century so much has been practically done as in the management of tumours of the ovaries and uterus. In ovariectomy alone we have abundant evidence of this, and hundreds of women are now living witnesses of the value of this operation who, only some twenty years ago, would

inevitably have perished. In the management of fibroid tumours of the uterus such great progress has not been made, though even here much has been done. We are now much more conversant with the structure and history of such growths; we know how they grow and are nourished, have watched their occasional spontaneous disappearance or cure, and have studied the mode in which nature effects it. Although all this knowledge may not have borne fruit with regard to treatment, still it is certain that much good seed has been sown, which will certainly in due season come to maturity. One of the mooted points in the natural history of uterine fibroids is, as to whether they ever of themselves entirely disappear. So many cases have been recorded by physicians whose evidence is beyond suspicion, and whose skill in diagnosis cannot be questioned, that it seems to us to be proved, strange though the fact may appear, that the spontaneous disappearance and absorption of even large fibroids has occasionally taken place. Observers are so apt to doubt facts that have not come under their own cognizance, however strong the evidence may be, that it is satisfactory to find the possibility of the occurrence vouched for by so good a witness as Scanzoni, and, as it is only by the narration of authentic histories that a doubtful fact of this kind can be proved, we quote his observations on the subject.

“Although such cases must be very rare, an observation which we have made proves the possibility of this termination (spontaneous absorption). In this case a fibrous body, the size of a man’s head, the diagnosis of which was perfectly sure, disappeared during confinement in a manner so complete that six weeks after parturition we could no longer discern a trace of this tumour, which had existed for eleven years. If we consider the great vascularity, the hyperæmia, the infiltration, and the softening which these tumours present during gestation, it will be seen that, if there is any time at which the conditions are favorable to absorption, it is surely the puerperal state.”

There can be no doubt that the occurrence of pregnancy greatly favours this change, and the explanation of this will probably be found in their texture, and in the fact that they are formed of structures similar to those composing the walls of the uterus itself. The process of involution by which the hypertrophied uterus is reduced to its normal size probably extends to the tumour, and gives the start, as it were, to its absorption. That medicine alone is capable of producing absorption no one, so far as we know, has ever stated, and it is certain that we have no means at our disposal to aid in so desirable a result. It is surely, however, not too much to hope

that, by a careful study of nature's process, we may eventually arrive at some means of favouring or starting absorption.

The surgical treatment of non-pediculated fibroid tumours causing severe hæmorrhage is a point of great interest. It is quite certain that in favorable cases much may be done to abate the exhausting hæmorrhage by some such means as incision of the cervix, or of the tumour itself, or even to cure the disease by avulsion of the tumour, as has been frequently done with remarkable success by Matthews Duncan. But with regard to the rationale of these methods of treatment, the precise indications which justify us in resorting to them, or the best means of carrying them into effect, we are still in want of accurate information, and none of the authors under review contribute much to our knowledge on these points. The operation of gastrotomy for the removal of large uterine fibroids, involving, as it frequently does, the removal of the whole of the uterus and its appendages, is a procedure which has been naturally enough looked upon with disfavour by most gynæcologists. Although it has of late been practised more than once with success, still it has not been done sufficiently often to admit of our arriving at any reliable conclusions as to its mortality, or as to the circumstances which justify a resort to it. We mention it chiefly because Thomas prophesies for it a much greater success in the future, and compares the present opposition to it to that which so long retarded the progress of ovariectomy. Time alone will show whether this opinion be justifiable or not, but the history of ovariectomy certainly teaches us not to dogmatise too decidedly on the subject.

The operation of ovariectomy is now so firmly established, and so constantly and successfully practised, that the opposition with which it was received only a few years ago is almost forgotten. It is unquestionably the greatest surgical triumph of the century, and English surgeons will always look back with just pride on the share which they have had in establishing it as a justifiable and legitimate operation. It is satisfactory to find that year by year this fact is being more and more widely acknowledged, and that France, Russia, Denmark, and many other countries, are following successfully the example set them in this country. Much valuable information on the subject will be found in Hewitt's, Thomas's, and Courty's works, which will well repay careful study. Time will not permit us to enter into the consideration of the topic, which would require more than one review devoted to it alone. We cannot, however, better close these necessarily imperfect remarks than by quoting Dr. Hewitt's summary of the results of the operation, results which are not matters of opinion, but of

simple facts, and which form a triumphant answer to those who still look upon ovariectomy as a procedure too hazardous to be undertaken:—"Of 100 individuals coming before us affected with progressive ovarian cystic disease, 90 may be expected to be dead within two years if nothing beyond palliative measures be adopted; out of the same number, 60 to 70 may by ovariectomy be saved, and saved permanently, from death."

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III.—Recent Medico-Psychology.¹

It is not easy to apprehend the real state of thoughtful opinion, as regards insanity, from the current works on the subject. Every writer, indeed, professes to look on insanity as a disease of the brain, but when cases are quoted in illustration it becomes at once evident that a various and wide diversity of opinion prevails.

A very natural consequence of the revolution of doctrines, by which alienist physicians were brought to regard mind as closely connected with material processes, and as not by its very nature warning off the inductive observer from inquiring into its operations, was that the influence of other material agents upon the form of matter that underlies mind drew an excessive amount of attention. If mind were but matter, it followed that mind must share the common fate of matter, and be at the mercy of other material forces that might happen to come into collision with it. Hence some could see in mental life nothing but a feeding of the brain from the blood, and in mental disease nothing but a derangement of the circulation in the skull. Hence also came a frequent tendency to see in insanity only a travelling of diseased processes from other parts of the body along the nervous paths to the brain.

When the "material" theory of mind first came into vogue it seemed that writers could not be "material" enough, unless

¹ *Body and Mind, an Inquiry into their Connection and Mutual Influence, especially in reference to Mental Disorders; being the Gulstonian Lectures for 1870, with Appendix.* By HENRY MAUDSLEY, M.D. Lond., &c. &c. London, 1870. Pp. 189.

Insanity and its Treatment: Lectures on the Treatment, Medical and Legal, of Insane Patients. By G. FIELDING BLANDFORD, M.D. Oxon., &c. &c. Edinburgh and London, 1871. Pp. 435.

The Pathology and Therapeutics of Mental Diseases. By J. L. C. SCHROEDER VAN DER KOLK, &c. Translated from the German by J. T. RUDALL, F.R.C.S., Surgeon to the Melbourne Hospital, &c. London, 1870. Pp. 158.

A brief paper on the Pathology of Insanity. By R. C. SHETTLÉ, M.D. Reading and London, 1869. Pp. 35.

they displayed this new mind-endowed tissue as at the mercy of every other neighbouring form of matter from the faeces in the colon upwards: such a thing as a series of actions almost purely in this tissue never occurred to their thoughts. And even still we can see, in a reluctance to acknowledge their bugbear "idiopathic" insanity, and in a certain shamefacedness with which a "moral" treatment is confessed as really sometimes beneficial, the same imperfect conception of what a cerebro-mental theory really involves.

But we should be misunderstood if supposed to maintain that this bias of alienists towards one phase only of the material life of mind was wholly a misfortune. We owe to it many beautiful investigations into the circulation and the circulatory apparatus of the brain, many skilful separations of individual groups of insanity, many general rules of health to which every thinking physician attends, and which he enjoins on his head-working patients. The treatment of insane patients with gastric catarrhs, hepatic congestions, loaded intestines, disturbed menstrual functions, has risen, under this bias of thought, from one of blind counteraction of the mischievous tendencies of the patient, or of blind and violent action upon some organ, rightly hit upon mayhap, but wrongly considered as containing in itself the whole "spirit" of the morbid affection, the devil that was turning the house upside down, to one of intelligent recognition of the importance of a gentle but firm effort at removal of one factor of the disease. The keeper in one instance, the fanatic in the other, has become the physician. And the very frequency with which this "sympathetic" insanity can be demonstrated proves, moreover, that we may often put our finger on an essential mark of insane neurosis in any particular patient when we say that the barrier between the operations of the non-cerebral and the cerebral tissues of the body is in this person broken down.

If we may venture at this stage to assign the positions of the writers here under review (as shown in their works quoted above), we should say that the crude view of an embodied mind, which sees in it only an appendage of the cerebral blood system, is exemplified to no inconsiderable extent in Dr. Shettle's pamphlet; that Schroeder Van der Kolk is enslaved by the same oppressing regard for the subordinate relations of the nerve tissue; this flows, however, in his case, a good deal from a metaphysical theory, which, placing mind in its highest functions beyond matter, leaves the nervous very much on a par with other bodily tissues. Dr. Blandford is by no means inclined in a similar direction, and always gives full weight to the proper mental relations (*cerebro-mental* relations, as we should prefer to call them) of the brain-mind; only we fancy in his case there

can be detected some shade of doubt as to whether he is not rather heretical when he advocates attention to cerebro-mental influences (mental, as he would call them, and the word is misleading for an alienist), a doubt grounded on too slight consideration, as has been said, of the cerebro-mental theory. Dr. Maudsley's writings are the embodiment to us of the truest and most clear-sighted view of all that this same theory means and involves, and he seems, therefore, to have aimed much the nearest shaft in the direction of a full scientific knowledge of insanity.

The last author's arguments for an "individuality of nerve element" ('Physiology and Pathology of Mind,' p. 375), and the recent discoveries of many subordinate individualities, will justify us in annexing a short consideration of those cerebro-mental functions which are most apart from vegetative processes, and stand most in need of a champion for their comparative independence.

The seat of *emotion* is placed by Dr. Maudsley, and also, if we read him properly, by Dr. Blandford, in the ideational centres; by Schroeder Van der Kolk in the upper and posterior parts of the brain-cortex. The question is not one of pressing importance, and, we believe, from regarding the diffusion of emotions (to which additional gray cells may contribute), that the truth lies somewhere between the two. But we pass on to more apposite points, the disorders of ideation and volition.

Under the head of "Symptoms of Insanity," Dr. Blandford commences with an account of the origin of delusions. One or two quotations may show its tenor:

"As insanity implies a deviation from the normal mental condition of the individual, so it connotes a physical disturbance of the brain function, with impaired sleep, possibly pain, heat of head, flushed or pale face, suffusion of eyes, throbbing of carotids, and such-like symptoms of cerebral disorder. This is the period of emotional alteration visible to others, of which the patient himself may or may not be conscious."

"He may be quite aware of it, and may seek advice like any other patient. He is more likely to be unconscious of his real condition, and to attribute the feeling he experiences to external causes" (p. 141).

These external causes constitute the predicate in the delusions. The patient, according to his own belief, is ruined, is damned, is bewitched, galvanized, and so on. This description seems hardly comprehensive enough; it may be taken along with that of Van der Kolk, given very briefly as follows:

"The cells of the cortex of the brain react certainly on stimulants,

but the induced perceptions are not manifested as feelings of pain, but in the form of ideas or pictures, of inclinations, of dispositions, of humours, according to the different locality of those cells" (p. 63).

This statement, though strongly tinged with Van der Kolk's belief in the transcendental nature of the faculty of judgment ("The higher intellectual faculties are thus not connected with these cells and their action, they are utterances of the mind itself"—p. 20), and also embodying the views above referred to under the head of "Emotion," is nearer what we think the truth than Dr. Blandford's. It seems to us that the physical basis of a delusion is morbid action among the ideational cells and fibres; that at the time when delusions are arising the physical basis of the state is the same morbid action, along with another in the emotional tracts, and which we see as "emotional alteration," just as Dr. Blandford states; but here we must stop, and no anxiety to prove to our unaided senses a material change going on should lead us to believe that "pain, heat of head, flushed or pale face," and so on, are essential to the appearance of delusion; otherwise we are sure in practice to be disappointed. These often are, no doubt, the exciting causes, but, as such, they do little more than take rank with other extrinsic matters, such as fright, sudden joy, the sight of much misery. Any one of them, or all of them combined, may be present, and are so thousands of times, without any delusion resulting; and the only constant antecedent of delusion is precisely what, in its material phase, we cannot yet put our finger upon, viz. the morbid condition of the centres that corresponds to a morbidly emotional state. That being granted—and unless it is so the proof of the immateriality of the changes accompanying the access of delusion would not be difficult—we are ready to acknowledge that in many cases the signs adduced by Dr. Blandford can be detected, while the picture he draws is certainly that of the origin of a very large class of delusions. The true physical history of delusion, the continued reproduction of the scar of the mind, has been long ago given by Dr. Maudsley.

Bound up with the relations of the *will* to insanity is the wide question of the existence of a moral or volitional insanity existing *per se*. As far as alienist physicians are concerned, the question may be regarded as answered. Even Dr. Blandford, who believes that "will is not one of the primary divisions of mind" (p. 33), and that "willing can do very little *per se*," and who gives an excellent analysis of Pritchard's cases of "moral insanity," showing involvement of the intellect in most of them,

acknowledges the existence of this variety, and admits two of Pritchard's cases as examples. And, indeed, apart even from the occurrence of undoubted cases, the possibility of continually irregular conduct, without expression of any delusion, seems as great as need be wished for. Ideational development cannot proceed apart from emotional development. Even in the case of a purely intellectual mind there is always present that "psychical tone" adduced by Griesinger, and which may be expressed as a continual hidden current of acknowledgment that this sort of life is the best for the brain of the subject of it. Emotion is the expression of the prosperity or of the adversity of the brain tissue, in whole or in part. If, now, a man should, either by inheritance or by force of circumstances in early youth, have those parts imperfect in which the current of social emotion, social feeling, pleasure in pleasing, pain in displeasing others, runs, he never gets the length of forming any ideas, nor of having any formal delusions on the subject, for one condition of the development of these is gone. At best, his delusions are utterly negative in their character. He does not see any harm in breaking windows, nor in stealing, nor in assaulting. The puzzling part of it (especially if a jury has to be persuaded) is that he may on some occasion express contrition for his fault. He has, in fact, found that an advisable course before, in order to escape punishment. Nay, he may feel contrition in this sense, that he anticipates punishment, and is pained accordingly. In fact, he is at the stage of the savage, and is commencing, in so far as he has this anticipation, his moral education. But he is generations behind the judge on the bench and the jury in the boxes; they inherit the long experience of healthy ancestors, by which the relations of men's acts to the general welfare have gradually been impressed on individual brains, and are now organized thoroughly, from the cumulative effects of successive legacies in these their latest heirs. Thus it is they cannot realise a man's acting as the criminal does, without there being at the bottom of it a positive contradiction of the validity of these organized ideas, these moral judgments, a reason for acting against them—in other words, a delusion.

We sympathise with Dr. Blandford in his dislike to such a term as "volitional insanity." Volition does well enough, in obedience, namely, to the feelings of selfish gratification that are present. It is in the absence of certain feelings that the root of the evil lies, and we prefer, therefore, the term employed by Dr. Maudsley, viz. *affective insanity*.

Thus, remaining in the field of pure nerve element, and trusting to nature's example in setting up a barrier between its action and that of other tissues, we may discuss some of the

main points in the old psychological order. But we now turn gladly to that field where the triumphs of modern thought and investigation lie, and review firstly the etiology, the rise and progress, of insanity in the individual.

And, above all, if any conclusion has been arrived at it is this:—That the causation is to be looked for, in very many instances, indeed, not in the individual, but in the stock. A gradual process of degeneration in the nervous tissue becomes established by which member after member glides out of the healthy social life, becomes thoroughly insane, dies in an asylum. The springs of life, of procreation even, are vitiated; the individual lives foolishly, marries foolishly, educates his children badly, lapses into insanity, or, at any rate, leaves his descendants in a worse position than he himself had at first. They in their turn repeat the errors tenfold. Soon the last member dies an idiot. But this is the blackest case, and all intermediate gradations may be found. While attractive qualities or selective powers still remain, one or more members may, by intermarriage, get for their children a better constitution; these, again, or most of them, may continue the happier course, and thus a healthier stock be gradually formed. In the individual elements of these series, eccentricity, insanity, and not these only, but many other forms of neurotic lesion, epilepsy, eclampsia, chorea, ataxy, neuralgias, anæsthesias, hyperæsthesias, may be found and are to be explained by reference to the inherited constitution as much as or more than to the particular circumstances of the case. Such is one great lesson which we have derived from Dr. Maudsley's teaching, and which we find always fully taken into account by Dr. Blandford. The pathology of this, the neurotic temperament, has not been exhausted by mentioning a list of diseases. Many marks testify to the degeneration in the individual, and these are excellently summed up by Dr. Maudsley in 'Body and Mind.' We would gladly quote the paragraph; but space hardly allows, and, at any rate, every one should read Dr. Maudsley's work. To the frequency of nervous lesion, adds itself a peculiar conformation of skull, of ear, of face, one or all; a curious look, a tendency to spasmodic, awkward movements, and many other signs more or less recognisable by the physician.

We would direct attention, in passing on from the causation to the actual disease, to two excellent passages in Dr. Blandford's work which deal with the treatment of this diathesis and with that of the state preceding an outbreak in a subject of insane neurosis. The first comes under the heading "Causes of Insanity" (Lecture VI), and the second, on referring to the top of the page, we find to figure under "Melancholia."

When the disease fairly breaks out in a patient it may take very different forms, and the naming of these forms, the classification of insanity, is one of the great troubles of modern medico-psychology. No man's brain is like another's. True, degeneration assimilates its victims, tending to land all in the same terminus, which in the last resort is, we suppose, the addition of a little carbonic acid and ammonia to our atmosphere or soil, but in the intermediate stages how varied the phenomena! Especially at first, when the more highly organized minds begin the descent of the downward slope, the confusion of individual behaviour is enormous. With difficulty we seize on the fact that a morbid emotional tendency often characterises the first stages, and we fling all such cases into two vast classes, viz. where there is exaltation and where there is depression. But we have achieved little and left much behind. Where there is this modification, the disorder produces the wildest acts in one patient, in another the wildest delusions, according, we may suppose, to the previous organising power and tendency in the brain, or to the preponderance of immediate muscular response to present feelings. These two sorts of brains are very different, and, we believe, justify Dr. Maudsley's two classes of affective and ideational insanity; the latter may be masked under the former for some time, but we cannot agree with Dr. Blandford that, because the transition does or seems to take place, the division is, therefore, to be discarded. On the contrary, the ideational phenomena are the best mark we can get of the standard which the patient's brain has reached, and of its quality as regards the direction of emotional diffusion, and these are weighty and essential particulars in the history of a case. Here, therefore, we have a second great distinction, and it is difficult to say whether it or the former one should be put highest. Griesinger took depression and exaltation as the leading marks, and brought in under them the contrast of ideational and affective insanity; Dr. Maudsley takes the latter as primary, and subordinates to them the existence of depression or of exaltation. According to the former plan a third class ("psychical weakness," including imbecility, idiocy, &c.), has to be constituted, which is *not* correlative with the others, while according to the latter all the members of that class can be drawn together under the heading of ideational insanity, albeit it is often rather an absence of ideation than a derangement of it that is present.

The attempt to classify the forms of insanity from the causes which excite the outbreak (insanity of puberty, of phthisis, of sexual excess, of dysmenorrhœa, of the puerperal state, of the climacteric period, of old age, of epilepsy, of gout, of chorea) is

a praiseworthy one, but is seconded by nature only to a limited extent. Were brains even tolerably similar to one another, they would present similar reactions on the same stimulants being applied; and in childhood and youth, as well as in old age, at which periods similarity is greatest, these causal divisions are more useful and better established. But the variety of adult cerebral organization is so great, that the result of the conjunction of this with any one cause produces, over a number of individuals, the most various results, and we have to fall back again on some attempt to classify from the mental side. It must always be remembered, too, that, after all, this causal division is but a classification of the causes of insanity, and is no more to be put forward as a classification of insanity itself than is the arrangement of elements and compounds by a chemist to be adduced as a sufficient classification of minerals. Whether it might serve both purposes is at the mercy of nature, and nature has decided in the negative. It seems to us that, before a satisfactory classification can be arrived at, the capacities of the individual insane mind will have to be more taken into account, and a series arranged from idiots and thorough demented (many epileptics must, psychologically, be put in with these), through imbeciles and partial demented, morally insane patients, and monomaniacs, up to the simply melancholic and the truly maniacal. The last term being applied purely to cases of the "general ideational insanity" of Maudsley, this series would convey a rough idea of the furnishing of patients as regards ideas, which may be taken as a fair test of the organization of cortical brain tissue. As secondary bases would have to be considered, the state of emotional tension, and its quality, giving mania (in the ordinary sense, "furor") and maniacal melancholy in their various degrees of severity; simple melancholia and simple exaltation of spirits (a frequent phenomenon in those otherwise insane, and in states bordering on insanity), which are put here because in them ideas are present, but weak and overruled; monomania with all degrees of violence or of suicidal tendency; moral insanity with all degrees of violence; imbecility, idiocy, and dementia, with low or high spirits, with obtrusive violence or quietude.

Schroeder Van der Kolk's classification is as characteristic as anything in his book, and shows him in his true light as strictly a physician to the insane. He divides into idiopathic and sympathetic insanity. The latter may be defined, according to his practice, as that sort in which there is either evident connection between some bodily disorder and the insanity, or in which drugs directed to some particular part other than the brain—stomach, colon, sexual organs, &c.—make a decided impression

on the mental disease ; the former includes all other cases. He claims for his method a practical value, and such, no doubt, it has. But Dutch patients must have peculiarly lowly brains if the alliance between lesions of the colon (which holds the place of honour among causes of sympathetic insanity), the prostate, uterus, &c., can cause madness to the extent which we would be led to believe from this author's account. Even "mothers who, through loss of their children, or even through vexation and ill-treatment which they had received from their own children, had fallen into melancholy," appear under the banner of colon insanity. This seems certainly like stretching a point ; but, apart from the question of classification, it behoves us to confess that the anatomical skill with which the author goes into the nervous and vascular connections of the sympathetically affected parts, and from which we learn much that we had not known before, at least in such a connection, as well as the simple honest enthusiasm with which he sets about the battle for cure, in the strength of aloes and myrrh, of injections *per anum* and hip-baths, win in us a regard for him which makes us rejoice in the excellent results which he humbly enough records. We read with cheerfulness of the young man "of very pale appearance," in whom "the onanistic tendency was removed by cold washings, derivations in the neck, and remedying the retention of the stools ;" and new hope dawns as we learn that "from his false religious ideas he was afterwards freed by preparations of iron." Indeed, Van der Kolk is to us, withal that he worked in a special department, the very ideal of a physician, armed with a thorough knowledge of the anatomy of the human body, keen to detect a point on which he may rest his Archimedes lever of medical skill, enthusiastic in regarding the results of his treatment to the extent of attributing everything to it, yet never to be accused of making quite baseless assertions, even in that respect.

"Even his failings leaned to virtue's side."

While thus we attempt to place in its due relation to others, to classify our case of insanity, what is going on within the brain of the patient, what is the pathology of the disease ? Very wide or very narrow is the answer to this question—wide, if we were to state, in reply, all the disorders that have been found to be bound up in the eccentric parts with the central mischiefs ; narrow, if we were to attempt to say what that central mischief is. The eccentric pathology we know fairly from the work of such men as Van der Kolk and Maudsley. Thus, from the former we gain the following quotations under "Acute Idiopathic Mania" and "Chronic Idiopathic Mania" respectively :

"The pulse is not unfrequently full and hard, the head hot, and the countenance is often strongly reddened, and generally rather swollen; the conjunctiva is often injected, the eyes quite glistening, and the pupils mostly small and contracted; the patient is in continual motion; he can just as little sit still as any one in a violent fit of anger or rage, and he often displays therewith an extraordinary amount of muscular power."

"Now more and more the signs of chronic congestion or of meningitis appear in the countenance; it has no longer a uniform deep colour, but the tip of the nose is mostly darker, and in a higher grade of chronic inflammation, the region above the eyebrows takes part in this darker colour (p. 34). Generally the region of the vertex feels hotter, and, indeed, also the forehead or the back of the head; whilst, always in proportion to the more or less chronic course, the hands and feet are cool, at the same time the hands also appear somewhat swollen and bluish. If one presses between the back of the head and the first vertebra, the patient very often feels an unpleasant oppressive pain in the head. This phenomena (*sic*) indicates an irritation and overfilling with blood of the medulla oblongata, which usually diminishes or even disappears by cupping the neck. Frequently the ears are seen reddened, which appears to proceed from congestion at the base of the skull and in the vertebral arteries."

Sympathetic insanity has its eccentric pathology pictured in a similar way, and we could give equally striking representations from Dr. Maudsley's writings, though not so much from his present work, which concerns itself more with other aspects of insanity.

But as regards the morbid process in the centres, little is known. The revelations of post-mortem examination, though immensely increased comparatively to our ignorance by the labours of Lockhart Clarke, Bucknill, Sankey, and others, in this country, and of Westphal, Rokitansky, and many other German pathologists, are still exceedingly meagre compared to the demand for even an approximate pathological history of insanity, as far as the nervous centres are concerned. Still, we are too apt to forget what we really have got, and to remind us of this we cannot do better than refer to Dr. Blandford's excellent summary. We must preface, however, the expression of our strong objection to his preliminary statement, that "all the records of former autopsies made in this fashion" (with the naked eye) "are as so much waste paper." Far from it! The senses of the true observer note truly, however ignorant he is of the deep meaning of what he sees. Musical ears could distinguish quarter tones long before it was discovered that these represented so many actual vibrations, more or less. The statement that "the brain-matter seemed reddened" by a Gairdner or

Handfield Jones may be worth much more (viewed, of course, in the light of careful microscopical research) than the assertion of a man who believes in the omnipotence of his "Smith and Beck," and with rash confidence proceeds armed in its strength to the rape of Nature's secrets.

The appearances then (which we have not space to quote at length), are such as the changes in the muscular coat of the small cerebral vessels, observed by Sankey in demented; the "kinked" appearance of these vessels recorded by the same observer from the brains of general paralytics; the existence of abnormal perivascular spaces in the white matter, optic thalamus and medulla of a general paralytic, as recorded by Lockhart Clarke; pigmentary degenerations, fatty degenerations, calcification, atrophy, proliferation of neuroglia (often beginning from the walls of the vessels), white spots in the white nerve tissue, called "miliary sclerosis" by Rutherford, amyloid and colloid corpuscles. The neuroglia or fine connective matter of the nerve centres is one of the most rising tissues in pathological importance.—All these appearances are found in the chronic insane, and to them can be added little more than some congestive or inflammatory states, local or general, on behalf of acute insanity. Can we, then, picture from these what is going on in the patient just become insane? Hardly; but we can at least agree with Dr. Blandford that "the nutrition and metamorphosis of the brain are in parts impeded or rendered irregular." The functional changes which accompany such a state are wholly unknown to us, for the very physiology of nerve function is unknown. We would, however, lay weight on the fact that, according to the cerebro-mental theory, the uniformity of nature is here also, and that the *mental* manifestations, can we only determine them with approximate certainty, are marks of underlying physical processes, and not to be altogether cast aside. Witness what we hope is now tolerably settled, the pathology of that well-marked form of mental aberration, general paralysis, the constant proliferation of connective tissue, and shrinking of nerve element in the *lamina nervea* of the gray cortex, which would seem to underlie constantly this definite mental state.

We have already indicated the views of our authors as regards treatment. Dr. Blandford is strong in prophylaxis, Van der Kolk in drugs. The former author gives likewise many excellent practical directions and hints in his closing lectures.

It must be remembered that all insanity is not of the hereditary sort we have taken as the type. Fresh cases are continually cropping up, and form, perhaps, about half of all that are admitted to asylums. The factors in the cause of such

acquired insanity are treated of by Dr. Maudsley in his second chapter, and are shown to be such as explain the result without reference to any supernatural process. On these cases depends the growth of insanity over and above what increase in population would lead us to expect—a growth in which all these authors are believers, and which an examination of the figures of dissentients will not disprove.

In taking leave of these works we must offer one criticism on Dr. Blandford's lectures, viz. that they want exactness and system, both in the arrangement of the matter and in the way words are used. The lectures on "Pathology," for instance, are a medley of forms of insanity, symptoms of these forms, and (necessarily) vague statements of the state of matters in the brain which can give little assistance or satisfaction to the student. On the other hand, the "symptoms" of insanity seem to descend no lower than the sense of touch in the scale of faculties and tissues. We simply quote the table of contents of Lecture IX:

"The two Extremes of Insanity.—Acute Delirium and Acute Primary Dementia.—Early Symptoms of Derangement.—Insanity with Depression.—Treatment, Medical and Moral.—Prognosis.—Melancolie avec Stupeur."

Fortunately, the author has compiled a really good index, but to see the strong points of the book it must be read through; more system will be required to render it thoroughly useful for rapid reference, especially in the table of contents and headings of pages, but also in the body of the work. The strong points to which we refer, and they are numerous, lie especially in the direction of the clinical delineation of insanity, and of real practical good sense in the lines of treatment proposed.

Van der Kolk's work is imperfect from the too early death of that talented physician. The greater part of it is taken up with the physiology and pathology of the brain, and with some thoroughly well-reported cases of pachymeningitis. The translation, though not perfect, is a most welcome gift from a "Surgeon to the Melbourne Hospital," and reflects much credit on Mr. Rudall for undertaking it.

Dr. Shettle's 'Brief Paper,' though, as we have said, too much occupied with the vegetative relations of the brain tissue, still shows no little appreciation of the proper scientific methods of inquiry in insanity. Considering the difficulty of dealing with so vast a subject in so small a compass, we are not surprised to find that Dr. Shettle hangs on to the skirts of various inquirers rather than draws a picture of his own. We dare say the pamphlet may be interesting to readers who care but for a morsel of a subject, and like that morsel good.

We have been able to do but scanty justice to Dr. Maudsley's 'Body and Mind.' It represents in itself a stage which the teaching in insanity has reached, and two essays which deal with the very foundations of Dr. Maudsley's scheme of insanity are bound up in the same neat volume.

How vast, the reflection occurs again, are the intervals which separate even thinking men in respect of insanity at the present moment! From the mixture of inquisitiveness and superstition displayed by the layman, to the hopeful unhesitating interference of the physician, grounded partly on metaphysical errors, to the stupid materialism of the follower of what is fashionable in philosophical medicine, to the materialism that dimly imagines itself right in holding on by old psychological forms, since some day they may turn up again at the top, to the philosophy that sees in Nature warrant for its advance and goes on fearlessly even in "material" paths, what strides, forward or obliquely, have we to take! We cannot conclude better than in a quotation from the highest teaching on the subject:—"What an unnecessary horror," says Dr. Maudsley in 'The Limits of Philosophical Inquiry,' "hangs over the word materialism!" "In the assertion that mind is altogether a function of matter there is no more actual irreverence than in asserting that matter is the realisation of mind; the one and the other proposition being equally meaningless so far as they postulate a knowledge of anything more than phenomena!" And again, that we may end with a call to progress, "The 'heaven-descended *γνωθι σεαυτον*' acquires new value as a maxim inculcating on man the objective study of himself."

IV.—Gigot-Suard on Herpetism.¹

THIS volume represents the ideas of the French school of dermatology, pushed to their utmost limits. The darts of diathesis of Alibert and his successors down to M. Hardy, the arthritic dyscrasia of M. Bazin, is here made the parent, not only of most diseases of the skin, but of those affecting the greater number of the other organs.

The humoral pathology which was prevalent during the last century prevented all progress in cutaneous medicine by the ease with which it substituted explanation for investigation. All our modern advances have been by means of minute anatomical study; for in this, as in other departments of medicine, a know-

¹ *L'Herpétisme: Pathogénie, Manifestations, Traitement, &c.* Par le docteur L. GIGOT-SUARD. Paris, 1870. Pp. 468.

ledge of morbid structure has helped most to the understanding of morbid functions.

"Since the itch-insect was discovered we have ceased to hear of *dyscrasia psorica*, or, if still met with, it is only among the disciples of Hahnemann." In like manner "the existence of an herpetic dyscrasia (*virus dartreux*, *diathèse dartreuse*), the mother of all sorts of cutaneous eruptions, has been disproved by a thorough investigation of their pathology, and we may regard it as defunct until its defenders succeed in expressing it by a chemical formula."¹ This is the language of the leader of the most sceptical school of "local" dermatologists; and the challenge it contains is almost accepted by M. Gigot-Suard.

Beginning with a short historical retrospect, he describes the supposed effect of the products of disintegration being retained in the blood in producing cutaneous eruptions, and cites cases in point from the presence of uric, oxalic, and hippuric acids, urea, extractives, &c. In some of these the *materies morbi* was given as a drug. Herpetism is defined as "a constitutional, chronic, hereditary or acquired, non-contagious, constant or intermittent disease, which depends directly upon the presence of these materials in excess in the blood." Its manifestations are not defined, but the bulk of the book is taken up with their description at length; and they include not only the so-called "dartres," as pytiriasis (*sic*), but affections like roseola and furunculus, certain forms of bronchitis and tonsillitis, of asthma and emphysema, dyspepsia, enteritis, amyloid degeneration, epilepsy and hysteria, consumption and cancer. The difficulty of tracing these and many other "manifestations" to the same prolific source of ill is not so great as it might seem; for the author does not attempt to give signs by which they may be recognised in themselves, as, for instance, may be the lesions caused by syphilis. In every illness the patient (or some of his relations) has had an eruption of the skin, and has suffered from what he supposes to be gout, or he has not. In the former case the "herpetic" origin of the malady is proved, in the latter there has been no herpetic diathesis at all.

In the vast number of observations which fill the book there is much to be found which would be valuable if recorded by an observer not intent on seeing his diathesis appear at every turn. It is more than probable that the bronchial mucous membrane is not the only one which is affected by gout, and that some forms of gastritis may be associated with excessive formation or diminished excretion of uric acid. Some of the cases here adduced are likely enough of this kind; but they

¹ Hebra, "Diseases of the Skin" (Syd. Soc. Trans.), vol. ii, p. 133.

suffer by being associated with others of a different character, and by the almost complete absence of any attempt to establish clinical features by which they may be recognised.

The author shows a good acquaintance with the works of English writers on the subject, and even a distant echo of German pathology appears from time to time to have reached him. Moreover, he utterly dissents from the retrograde opinions of M. Bazin on the relation of rheumatism to gout, and in his discussion of this subject shows sound sense, in addition to the industry and zeal which characterise the rest of the work. The chapter on the treatment of herpetism is a short one, and discusses the employment of colchicum and green coffee, and of various alkaline and other mineral waters; among which those of Caunterets, which contain sulphur as well as silicate of soda, naturally receive a due share of commendation, M. Gigot-Suard being "médecin-consultant aux eaux."

The last chapter is devoted to an account of experiments made on animals with the object of supporting the writer's theory. Five dogs were dosed with uric acid, and seem generally to have scratched themselves before they were killed. The organs presented those characters of congestion so familiar in the records of human pathology, but the joints were never found inflamed, and only once was urate of soda detected in the kidneys. Two other dogs were poisoned by oxalic acid; one suffered from diarrhoea, and a lung was found full of tubercles; the other showed but little effects, alive or dead. Three dogs were given urea in large doses; one only seemed the worse for it, and he was not killed; at the autopsy of another "nothing remarkable was found." One dog took two grammes of hippuric acid for twenty days, with no other effect than apparent irritation of the skin, and both dogs and rabbits were given ammonio-phosphate of magnesia with still more negative results. The author deserves credit for making these experiments and recording them so faithfully; but when, as is to be hoped, comparative pathology can be made to resolve questions insoluble by clinical medicine, we anticipate that the doctrine of an herpetic diathesis will receive its last blow.

V.—A Manual of Medical Jurisprudence for India.¹

Although it is not stated in the title-page, this is the third

¹ *A Manual of Medical Jurisprudence for India, including an Outline of a History of Crime against the Person in India.* By NORMAN CHEVERS, M.D., Surgeon-Major H.M. Bengal Army, Principal of the Calcutta Medical College, &c. Calcutta, 1870. Pp. xix and 861.

edition of a book, the second of which was published in 1856. The work, as it now appears, has been rearranged, and its original scope has been so greatly enlarged that it may be virtually regarded as a new book. The author warns his readers in the preface that they are not to expect an elementary treatise on medical jurisprudence. "It is a system for India, intended to be used by those who have already mastered the science of legal medicine as it stands well-nigh complete for Europe in the works of Taylor, Caspar, and Guy."

The principal subjects discussed in this manual (although why a gigantic volume like this should be called "a manual" we cannot conceive) are the characteristics of the criminals of India, the influence of custom upon crime, the search for the bodies of missing persons, rules for the transmission and examination of wounded and dead bodies, identity of dead bodies, the uncertainty of general evidence in India, simulated death, dying declarations, medical evidence, identity of the living, poisons, including *inter alia* arsenic and its compounds, aconite, datura, cannabis, opium, belladonna, nux vomica and strychnia, poisonous grains and legumes, &c., with remarks on cattle poisoning and thuggee by poison, wounds, hurts and personal injuries, lethal weapons usually employed in India, injuries inflicted by wild animals, bites of venomous serpents, human sacrifice, causes of death after injuries, torture, strangulation, hanging, suffocation, drowning, burial alive, suicide, rape, unnatural crime, pregnancy, criminal abortion, delivery, infanticide, and insanity.

As it would be impossible to do justice to the labours of Dr. Chevers in a single article of ordinary length, we shall postpone to a future occasion our observations on the subject of toxicology, to which more than two hundred pages of his ponderous but, at the same time, very original volume, are devoted.

In his introductory remarks Dr. Chevers quotes the opinions of two great lawyers, Sir J. Mackintosh and Lord Macaulay, regarding the native character of the two foremost classes of the people of India, and observes that—

"The operation of the great moral defects laid bare by these stern judicial critics, in stamping a peculiar and distinctive character upon the crimes most frequently committed in India, is abundantly evidenced in every page of our criminal reports, in which thefts, perjury, personation, torture, child stealing, the murder of women and aged men, assassination, arson, the butchery of children for the sake of their ornaments, drugging and poisoning, adultery, rape, unnatural crime, the procuration of abortion, are among the leading villanies."

The author has endeavoured to ascertain the comparative

frequency of crimes among the Hindu and Mahomedan inhabitants, and his investigations tend to show that there is a large excess of crime against the latter, if we take into account their relative numbers. In many forms of crime, as *Dacoitees* (gang-robberies), *Dungas* (faction fights), and *Thuggee*, Mussulmans and Hindus are often found associated. Although the proportionate frequency of certain crimes varies much in the different parts of the country, there is no conspicuous distinction in the characteristics of crime as practised in Bengal, Bombay, Madras, the north-west provinces, or the Punjab. In the East, as at home, *custom* is inextricably interwoven with the native's religion. "As a few years ago our best men in England followed the wicked old traditions of their order, and were at any moment liable to be forced into fighting a duel, so the Bengalis and the Rajpoots of the present day are tempted to commit certain atrocities *more antiquorum*."

In the chapter on "Search for the Bodies of Missing Persons" there is much singular information. From the physical characters of the country, and from the fact that in many districts human remains are abundant on most river banks, the discovery and identification of the bodies of missing persons must be difficult; and this difficulty is largely increased by the selfish timidity of the natives, who from their dread of a judicial inquiry avoid coming forward as witnesses even when they have seen their own kindred murdered. Thus, in 1856, a man at Bareilly, whose child, four years old, had been strangled for its ornaments, was persuaded by his neighbours to report to the police that a wolf had carried her off and eaten her. Many similar cases might be adduced, showing that from sheer terror, or in some cases for a reward, persons have consented to forego the prosecution of the most atrocious criminals.

Another difficulty in the way of the police in searching for murdered bodies is that the latter are often removed several miles from the scene of the crime. Thus, in a case of dacoity with murder, in 1855, the corpse was found three days subsequently on a plain ten miles distant from the place where the crime was committed; and many instances are on record in which the body has been thrown down a well five or six miles off.

To prevent traces of blood leading to their detection, murderers have been known to kill a calf afterwards at the site of the crime. Foot-marks sometimes afford a clue to the police; and we may incidentally remark that the *forms of natives' feet* are equally worthy of study by the ethnologist and the medical jurist. In climbing and in executing many kinds of "handiwork" various classes of natives use the foot with great

precision and power as a prehensile organ, while hill-men have the great toe so much developed and set wide apart from the others that their footsteps can be readily distinguished from those of dwellers in the plains.

Murderers who have not the advantage of residing near a river often conceal the bodies of their victims in dense jungles, where birds and beasts of prey and insects soon reduce them to skeletons. In a jungle cleared in 1865 seven skeletons and one recent body were discovered. Wells afford excellent hiding-places. Forbes states that during two years no less than sixty-seven bodies were taken out of wells in a single district. Both in Bengal and in the north-west provinces bodies are often buried in the mud floors of houses, a mixture of cow-dung and water being heaped over the spot, so as to obliterate any traces of disturbance; and the cooking place is frequently chosen as the most secure spot for concealment. In the north-west provinces, where wheat is abundant, dead bodies are often concealed in dwellings or outhouses under heaps of bran or chaff; moreover, in that part of the country the holes of porcupines afford ready places of concealment for the corpses of murdered bodies. Jackalls, dogs, vultures, and ants, have occasionally led to the discovery of dead bodies, and Dr. Chevers states that the common pariah dog of the country, and possibly also the vulture, might be trained so as to assist the police in this department of their duty.

In his chapter on "The Identity of Dead Bodies" the author, first gives the well-known case of Heasman (that occurred in April, 1868), in which, as our readers probably recollect, the body of a man apparently dead for two or three months was discovered in an unfinished house at Hackney, and identified by the physician from whose asylum he had escaped on the 1st of February, and by his brother; after which a Mrs. Banks erroneously claimed him as her husband, who had been missing three years. He then quotes several similar cases which were elicited by that of Heasman, and finally adds the following, which occurred within his own Indian experience:

"A. was a patient in my European ward, a portly Scotchman of rather striking appearance, approaching middle age. Some weeks previous to his discharge a tall stout man was brought to the hospital one evening dying from the united effects of alcoholism and insolation. B., having an interest in A., heard that his body was lying in the dead house. She went the next morning and saw the body, cried over it, and covered it with a sheet which she had provided for the purpose. Some one now suggested that the body might not be that of A. after all, and B. was advised to write a note of inquiry to A.'s address. He immediately replied that he was quite well. Here, most pro-

bably, the error arose mainly from the fact that in the Calcutta hot season the bodies of fat men certainly do become very much alike after decomposition has fully set in."—P. 47.

There is no place in the world, says our author, where it is so needful that the body should be (1) *produced*, and (2) *fully identified*, as in India. In illustration of this necessity he quotes two cases in which Englishmen have been tried for the murder or manslaughter of native boatmen whom they had knocked overboard, and who, being good divers and swimmers, had safely landed and then through their friends instituted a prosecution. Moreover, it seems to be a popular crime, at all events in Bengal, to cause a person to disappear and then to charge some obnoxious person with the murder, a putrid corpse from a neighbouring river, or a fresh headless body being often produced as that of the missing person. In a case tried in 1853 certain prisoners were convicted, on their own confession, of perjury, in having falsely deposed to the fact of a murder and to the burial of the corpse. At the conclusion of the trial the alleged deceased, Pertaubnarain, made his appearance in court. In his report of this trial the judge remarked that it would be impossible to imagine a case more completely satisfactory as regards the guilt of the accused until the appearance of the murdered man brought to light its true character. The prosecutrix was the mother of the missing man, and the principal witnesses were his wife and cousin, while the *prisoner's own servants* described the burial of the body. "There were," says the judge, "no inconsistencies and no contradictions in the evidence, which from first to last gave the hearers the impression that a heinous crime had at last been brought to light, in spite of a powerful combination to conceal it." Truly the heart of the Bengalee is desperately wicked; and who can penetrate into its secret depths?

The following case is only one out of several which Dr. Chevers has collected, in illustration of the necessity of finding and identifying the missing body. Told briefly, it may be thus stated:—Two boys set off on a journey from Bundlekund, and, while bathing together, one, for the purpose of robbery, threw the other into deep water, in which he was supposed to have been drowned. The supposed murderer confessed his crime, and was sentenced to transportation for life, instead of being capitally convicted, *as the body had not been found*. When the proceedings were before the Agra Sudder the boy supposed to be drowned returned to his home, but did not come forward till the sentence had been passed. It appeared that he was found insensible by a worthy Brahmin, who took him home and nursed him.

Far more remarkable than the preceding case and those similar to it, in which the main peculiarity is the production, alive, of the person alleged to be murdered, is the following romantic history, in which not only does the accused confess to the murder of a living person, but the parents deny the identity of their daughter, of whom they had only lost sight for three years. On the latter grounds the case has at the present time (when the Tichborne trial is proceeding) an especial interest :

"In 1809, Nubia, the daughter of a respectable Hindu named Dhur, living in a village in the Doab, became attached to one Kulian. Her family being adverse, she eloped with the young man, taking with her valuables to a considerable amount. The father went to Cawnpore, where he found Kulian, but without the girl, and had him apprehended as his daughter's murderer. Kulian readily acknowledged that he had taken Nubia from the village, and, while under examination, he informed a friend that he had buried the greater portion of her clothes and the ornaments which she had carried away in the house which he then inhabited, pointing out the precise place, and requesting that they might be given up to her father. He at first preserved a mysterious silence respecting the fate of the owner, but, the property being found, he was taken to the thannah. Here he admitted that he had killed the girl, stating that he had thrown her body into a nullah, and offering to show the police-officers the spot. Upon their arrival they searched in vain for the usual indications ; no remains of a body could be found, nor any signs of the perpetration of a murder. He then said that, being apprehensive of ill-usage at the thannah, he had pleaded guilty. He also asserted that he had left his companion alive and well in the neighbourhood of the camp at Cawnpore, but knew not what had become of her afterwards. At the trial the police positively declared that neither threats nor violence had been employed to induce him to confess. He readily admitted that he had eloped with the girl, and also acknowledged that he had confessed the murder imputed to him, but declared that he had been beaten and menaced by the police, and had, therefore, confessed through fear. He declared his perfect ignorance of Nubia's fate, saying that, when they neared the cantonments, they being afraid of being seen together, it was determined that he should go into the city and procure a house, and she entrusted her bundles to him. Leaving her beside a well until his return, they parted early in the morning ; on his return, at mid-day, she had disappeared. He was sentenced to thirty lashes, and to imprisonment for fourteen years, the proof being considered insufficient to warrant capital punishment. About three years subsequently, Medaree, Kulian's brother, brought forward one whom he alleged to be Nubia. She made oath that she was. The father and mother, however, denied her identity. Consequently, Medaree and the girl were sued for perjury. The father, mother, and an intimate friend of the family, who had been acquainted with Nubia from infancy, positively denied that she was Nubia. On the other hand, four witnesses were found who

had known Nubia from her childhood, who as positively testified to her identity. The family disgrace and the loss of caste which she had incurred created a suspicion in the mind of the court that Hindus of respectability might be unwilling to admit that a person known to have misconducted herself for years really belonged to them. She stated that, being tired of waiting at the well for Kulian, she set forward in search of him, and, while thus fruitlessly employed, met with a soldier belonging to one of the regiments stationed there, and, after considerable persuasion, was induced to place herself under his protection and accompany him home.

"The regiment being shortly afterwards ordered away, she had quitted Cawnpore, and had only lately returned, when, meeting Kulian's brother, she had come forward in his justification. No evidence was adduced in support of this statement, and the whole question rested entirely upon the probability of the parents denying their daughter, in consequence of motives which, under the circumstances, were very likely to influence them. The court also perceived a resemblance between the mother and the alleged daughter; and the testimony of the person calling herself Nubia being the individual whose identity she claimed, appearing to preponderate, she and Medaree were acquitted.

"The sentence of Kulian now required revision; he was only amenable to punishment for the abduction of an unmarried female, and for carrying off property of considerable value; but as he had voluntarily returned the greater portion, and, moreover, had suffered nearly four years' imprisonment, the punishment undergone was deemed quite adequate to the offence, and he was discharged." Pp. 51-2.

The remarks on the identification of the *religion* of deceased natives, especially in the case of headless corpses, are extremely interesting; but as they are of no practical importance in this country, we must content ourselves with referring to them. In Madras, as everywhere else, the *teeth* are, as Dr. Chevers remarks, frequently a clue to identity:

"One of the latest examples of this was the aid which the presence of a small pointed supernumerary tooth, between the middle incisors of the upper jaw afforded in discovering the identity of the body of Rose Brown, in Calcutta, in 1868. This, however, is not a very rare abnormality, for I have, without any search, observed it in three other instances within the last ten years. As the presence of *false teeth* led to the identification of Dr. Parkman's ashes, and of the skeleton of Patrick O'Connor, the Mannings' victim, so the body of the Prince of Benares, who was slain in battle by Cuttah, in the year 589 of the Hijera, was recognised amongst the heaps of slain by artificial teeth, which were fixed in by golden wedges and wires." P. 60.

In a case tried in 1830 all that was discovered of a murdered man was a portion of scalp with a *tuft of hair*, which imprisoned three of the murderers for life. The victim was a red-haired Englishman, known as Dick Sahib; he was carried

away from his own verandah by a band of armed men, and was never again seen by his friends. A rival of Dick's in the indigo business, one Gong, was suspected, and in his cow-house, two cubits below the surface, were found the fragmentary remains, to the identity of which Dick's native mistress swore. Red heads, it must be recollected, are comparatively rare in Bengal.

For the purpose of avenging himself upon an enemy a native will not only lay a wounded corpse (which is generally to be found if needed) at his door or in his verandah, with the view of bringing him into trouble, but if a body is not ready at hand, will murder a cripple, idiot, or aged person for the purpose. "Probably," says Dr. Chevers, "the most atrocious case of the kind on record is that of a woman in the Patna district, who poisoned her own little daughter, and, having concealed her body on the premises of a neighbour with whom she was at enmity, accused her of having murdered the child."

Until we had read this volume we had no conception of the difficulties an Indian magistrate or judge must experience in deciding on the value of native evidence. In his report for 1866 the Inspector-General of Police in the Lower Provinces states that "this is a country where perjury is the rule and not the exception, where no man will tell the whole truth or the simple truth, where exaggeration is perfectly natural, and where false witnesses can be bought for a few *annas*."¹ In one of the appendices to the 'Report on the Bengal Jails' for 1868 the author states that perjury and false charges are very common offences, but that the false charge is generally an extremely exaggerated version of something that really happened. He thinks that the false witness usually believes that the statements to which he swears are true in the main, and that "if he had been luckier he might have seen the things he says he has seen, or heard the things he says he has heard." This writer, who has clearly studied the native mind, thinks that the Cachar Bengalee would generally tell the truth if he were sure it would not injure himself or his friends, while the Munipoorie "never tells the truth if he can possibly tell a lie."

Another difficulty with which the district magistrate or judge has to contend is the *prepared* or *too perfect evidence* which is frequently offered. When a prosecutor swears, in a case of assault, that "the first prisoner, standing to the south, struck me one blow on the arm with a stick two cubits long and two fingers thick; the second prisoner kicked me with his right foot on my left leg," &c., he very probably has actually been assaulted, although he has drawn upon his imagination for the details with which

¹ The value of the *anna* is three half-pence.

he has embellished his story, or has learned by heart the exact evidence which he had been recommended to give.

Again, the confession of guilt must be received with extreme caution, for at least two reasons, which we shall notice *seriatim*. In the first place, cases are on record showing that, probably from their fatalistic views, natives, feeling that the evidence against them is strong, think that their shortest and easiest course is to confess, even when they are conscious of their entire innocence.

Some years ago the headless body of a woman was found in a well in the Mirzapore district, and suspicion was attached to some Rajpoots, whose sister, a widow of bad character, was missing. Upon investigation the men not only confessed the crime, but described the murder in all its details, and they would have been convicted if, when the case was pending before the Sudder, the missing woman herself had not appeared. She had eloped with a lover, and, accidentally hearing that her brothers were charged with killing her, came forward to clear them. At first her story was disbelieved; but as she persisted in her statement, she was ordered to be placed amongst the crowd in the court, when the prisoners were filed in, as if to hear their sentence. "Directly their eyes fell on their sister they raised a glad cry of recognition, and with tears of joy praised God, after their own fashion, that they were saved." The woman was then identified by others, and the brothers stated that "they had confessed because, finding facts, and that thing more terrible than fact—*rumour*—against them, they thought it was hopeless to plead innocence."

Secondly, until about fourteen years ago, when a new organization was instituted, the police were often charged with obtaining a confession by intoxicating the suspected person, or by the influence of more powerful agencies, as intimidation or torture. Several cases are recorded by Dr. Chevers in which prisoners have retracted confessions which they had made when, as they alleged, they were in a state of intoxication consequent on their taking drugged liquor or tobacco offered to them by the police; but whether they had been drugged, as they asserted, this physician thinks questionable. That various forms of torture have been adopted, both formerly and of late years, by the police and their subordinates, especially the latter, there is undoubted evidence, although such a procedure, or even intimidation, is severely punished if detected by the higher powers. The modes of torture are usually, but not invariably, such as leave no trace of injury, and some of them are horribly ingenious. In 1855 several members of the police were convicted of torturing a man into confession by tying his hands behind him with a cloth, and

hoisting him up to a beam by a rope attached to the cloth. In 1866 an inspector and sub-inspector were proved to have trussed four suspected persons up to the roof, keeping them without food and beating them with slippers. Mr. Dampier relates a case that occurred some years earlier, in which the police "tied together the fingers and toes of a man suspected of dacoity, and drove wedges between them to the greatest extent of tension. He was then laid out on his back in the sun; and this not producing the desired effect, his hands and feet were dipped in boiling water."

In 1869 an atrocious case of police torture occurred in Upper Scinde, and was reported in the 'Scindian,' whence it found its way into the Calcutta papers. A confession was extorted from a man accused of murder by muffling his legs in a cloth saturated with oil, and setting the cloth on fire. Torture by cold is sometimes resorted to by the police, and in 1866 a sub-inspector was transported for life for causing the death of a man suspected of dacoity, by pouring water on and then fanning his naked body on a cold February night. In 1855 a policeman at Ahmenabad was dismissed (lucky miscreant that he was) for having suspended a prisoner by the hair of his head to a beam, and for ordering him to be beaten while in that position.

Lifting by the moustache or by the ears are processes not unknown to the Madras police as means of extorting confession.

Stocks, constructed in various ways, so as to cause every degree of suffering, from mere discomfort to exquisite torment, are abundant in Bengal and Madras, and are, we fear, often applied to illegitimate purposes. A writer in a Calcutta paper states that he has seen them in all parts of Bengal, and that they have holes for legs of all sizes; so that if a stout man's legs are put into little holes, a confession quickly follows. He saw a young fellow in this state of affliction, consequent on his being found with a few *lotas*, for which he could not clearly account. He confessed, next day, that he had robbed his master, who, when the case came on, declared him innocent. "I have no doubt," says the writer, "he would have as readily confessed to having murdered his mother, for the stocks is a system of torture which a man soon gets tired of."

It is scarcely three years ago that a head constable and two of his subordinates were sentenced to five years' imprisonment and a fine of 200 rupees for torturing a servant of a railway station-master on a suspicion of theft. After beating him in several ingenious ways, and pressing his throat with a bamboo, they ordered two mole-cricketts (*Gryllus grillo-talpa*) to be applied to his navel, and to be kept in that region by being

covered with a shell, until they had grubbed their way into his body. As somewhat parallel cases, we may mention two instances noticed by Dr. Chevers, in which prisoners were placed and kept on ants' nests.

Such are some of the modes of torture which, we fear, even at the present day, are occasionally resorted to by the police, in their over-eager zeal to find a culprit, and cases are recorded in the police reports of "their determination to *create* a murderer when a murdered body is discovered." For example, about five years ago a corpse bearing marks of violence was found floating in a certain river. The head constable of Rungpore at once went to the spot at which the body was lying, and ascertained from Chepoo, a woman of bad character, that her adopted father, Ootum, was missing. On being shown the corpse she failed to identify it, but at length, after passing the night with the constable, and on the strength of certain promises, she consented to make a false identification, and declared the body to be that of Ootum. He then induced her to go further, and declare that one Bocha and four others were the murderers. At this stage of the proceedings a sub-inspector appeared, and relieved the head constable of the charge of the case. It was now evening, and the five persons named by Cheepoo were taken for the night to a certain house, where they were kept till next morning in the company of three constables. It was proved at their trial that during the night they were so tortured by their keepers that when brought before the sub-inspector next morning they all made a false admission of having murdered Ootum, and they were, on that admission and on the false evidence extracted from Cheepoo, forwarded for trial. While the magistrate of the district was actually engaged in holding the preliminary inquiry, Ootum himself appeared. The true history of the case was then elicited, the police and the prisoners changed places, and the former were, with the exception of the sub-inspector, sentenced to five years' rigorous imprisonment.

Other difficulties in the way of getting at the truth are due to the native habit of swearing to hearsay evidence and personating witnesses, and thus giving evidence by proxy. The following is a good illustrative case:—In 1837 a Bheel (we will call him A.) was tried for perjury. It appeared that in a trial for highway robbery a witness, B., was called for the prosecution, when the prisoner A. answered and personated B., and declared that, on a certain day, he had followed the footprints of certain robbers, &c. On cross-examination he broke down, and admitted that he was not B., but A., and that he was not present when the robbers were traced; but he urged that, as his friend B. was sick, and unable to attend, he (A.) came to

depose for him; that the facts to which he deposed were true; and that, though he was not, strictly speaking, an eye-witness to them, they were notorious to all his neighbours. He did not escape punishment for his perjury; but, strangely enough, if he were a Mussulman, he might have pleaded that "Mahomedan law, in certain prescribed cases, allowed the singular practice of giving evidence *by proxy*. In the event of the death of the principal witness, the absence of the witness on a three days' journey, or his sickness, a witness, or the person who would have been such, was permitted *to supply a proxy*, substituting another person to detail facts or opinions for him" ('Beaufort's Digest of the Criminal Law,' &c., section 635).

We have in a previous page referred to the fact that the natives will not unfrequently commit a murder for the purpose of throwing the guilt on an enemy. In some districts the process is slightly varied, and *suicide* is substituted for murder. Dr. Chevers devotes a section of his very interesting chapter on suicide to "Suicide for Revenge or Accusation." He tells us that "among natives the sacrifice of oneself or of a relation is believed to bring down vengeance on those by whose act such sacrifice is precipitated," and quotes the case of a sepoy who, influenced by this feeling, put an end to his brother who was wounded, when the police were apprehending them as deserters. Persons still occasionally sit *dhurna* (literally starvation, but it means "sitting by an enemy's door and dying by want"), although, since 1820, the act is punishable by fine or imprisonment. By this procedure they hope to bring down a curse upon those whose doors they beset. In the inveterate feuds that prevail among rival families or villages Up-country it is stated to be by no means a rare thing for an aged person to allow himself to be taken, a willing victim, into his opponent's land, when his own people murder him and charge their crime upon their adversaries.

The custom of committing suicide for revenge is, as probably many of our readers know, singularly prevalent in China; the Chinese law throwing the responsibility of the act on those who may be considered its primary cause. Hence a person desirous of being revenged on an enemy has only to kill himself, and he can do it with the consolatory feeling that he is sure to get his adversary into horrible trouble, including deprivation of his goods, torture, and probably loss of life. As special additional inducements for a Chinese to select this mode of *exit* from the troubles of this world, it may be noticed that the family of the suicide usually obtains considerable damages, and that public opinion honours and glorifies the act.

It might naturally be supposed that the most malignant

native could not carry his animosity farther than to destroy himself, in the hope of bringing down present and eternal punishment on his enemy; but we are almost inclined to think that one who with the wish to prefer a charge of murder against an obnoxious neighbour, would venture to simulate death and allow himself to be brought to a medical man for examination, shows even greater steady courage which, if exhibited in a nobler cause, would amount to heroism. Several such cases are actually on record; two were related to the author by the late Dr. Kenneth Mackinnon, and the following is given by Captain Bacon, who himself witnessed the scene, in his 'First Impressions of Hindustan.'

A squalid-looking corpse with two or three wounds on the chest, and with many marks of violence on other parts of the body, was brought into the house of a civilian. The magistrate of the district, suspecting the dead man to be simulating, was proceeding to examine the corpse, when he was earnestly begged "with a thousand importunities" not to touch it with his hand lest he should pollute it. He therefore stuck the sharp end of a billiard cue into the side of the corpse, a procedure that occasioned no marked effect, except on the feelings of the friends; a repetition of the blow with such force that the point penetrated the flesh between the ribs produced a very slight quivering of the muscles, and an almost imperceptible movement of the head, sufficient, however, to reveal the imposture. The people when told that life was not quite extinct, and that they had better take their friend to the hospital, exclaimed, "The man has been dead since cock-crow." A kettle of hot water was then brought, and notwithstanding the entreaties of the surrounders, a small quantity was poured on the foot, when the dead man "bounced from the charpai and fled like a spirit." As lately as 1869, a servant wishing to terrify his master, simulated death cleverly for many hours; but on an attempt being made to open the eye the orbicularis acted, and "a call for a red-hot iron had a surprisingly restorative effect."

As we have already remarked, we shall postpone to a future article our remarks on the subject of *Poisons*, and shall now proceed to notice the chapter which treats of "The Lethal Weapons commonly employed in India." While in this country they are virtually restricted to the gun, loaded bludgeon, or knife, the eastern list includes the bludgeon, club, pestle, sword, halbert, axe, pointed and split bamboos, panjees (slender bamboo spikes), pellet bow, spear, and bow and arrow; and the hand is (we think, incorrectly) placed by Dr. Chevers in the same category. The bludgeon or *lathi* "carried by every traveller and by every rascal bent on mischief, is a stout bamboo staff

of good weight, and generally measuring from four to five feet long. It is used as a walking-stick or is carried on the shoulder," and can inflict wounds "varying in intensity from the broken scalp to the crushed-in skull." This seems to be by far the most commonly used of all the weapons named, and next in order stands a variety of halberd (or battle-axe), with a crescentic blade attached to the thick end of a bludgeon. It is the usual weapon of watchmen or *chokidars*.

Although in former times the people of India, both Mussulmans and Hindus, were remarkable for their skill in archery, arrows are at present only used by the hill tribes, who often poison their points with a preparation of the *Aconitum ferox*. In 1853, a hill man was tried for the murder of a jealous husband who had chased him for above a mile, when he (the hill man) turned and shot the deceased with a poisoned arrow at a distance of *eighty paces*. The deceased never spoke after he was wounded, and died in the night. Several similar cases are on record, and the accuracy with which these people aim at the heart is very remarkable.

Both in the hill countries and in Bengal a very trivial cause of offence is sufficient to call down the most appalling punishment. For example, a woman of Sylhet when cooking was asked by her brother-in-law to grind down some herbs for him. On her asking him to wait a little lest her vegetables should be spoiled, he seized a very sharp bill-hook (*dao*), and cut her down, inflicting ten wounds on her head and neck. Two men of Allahabad had been eating together, and one told the other to take away the cooking pots. Upon his replying that he would do so presently, his companion seized a sword and slaughtered him with nineteen wounds. Many similar cases of furious homicidal violence on almost no provocation are recorded, and from their study Dr. Chevers comes to the remarkable conclusion that these frightful outrages, especially when committed with the *dao*, are done after a certain definite mode which must be the result of a design (or shall it be called intent?) "to split the skull, to cut off the head, and to lay open the heart and lungs."

Passing over a couple of chapters we come to the consideration of "The Injuries inflicted by Wild Animals." To those who have paid no attention to the subject the statistics of the deaths from animals in the East will probably be very startling. In Bengal alone the ravages of wild animals during the six years preceding the publication of Dr. Chevers's book in 1870 have been almost as bloody as a campaign. A tabular statement shows that in that space of time 4218 persons have fallen victims to tigers, 1407 to leopards, 105 to bears, 4287 to

wolves, 174 to hyænas, and 3210 to other beasts, such as boars, jackals, mad dogs, elephants, &c. The following curious case of an old Brahmin in a state of intoxication being attacked by vultures is worthy of record in our pages. While he was lying insensible the birds devoured the tissues constituting the whole buccal and labial region, the cartilages and septum of the nose, together with the tongue, uvula, and left tonsil, and picked the exposed maxillary bones nearly clean. This unfortunate old gentleman was fed by means of an œsophageal tube, and did not seem to suffer much except from the flies. He apparently did well from his admission into the hospital on the 9th of March till the morning of the 14th, when he was found dead.

There is much matter that will be perfectly new to a great majority of our readers in the chapter on "Bites of Venomous Serpents." After noticing the mortality caused by snake-bites, and the number of snakes killed in Bengal daily (the average being 4633 per diem between 15th October and 7th December, in 1863), Dr. Chevers gives Dr. Fayer's list of the best known of the poisonous land and sea-snakes of Hindustan. Of the former the cobra contributes chiefly to the recorded mortality, while all the pelagic serpents are venomous. There seems to be good reason to believe that a vast amount of undiscovered crime (at least in Bengal) is concealed under the report of "*deadly snake-bite*." Seven such cases were discovered in the Bogra district in 1868. It is a popular belief in Bengal that an habitual opium eater may handle venomous serpents with impunity, and that all the snake-catchers indulge freely in the use of this drug; while in Madras it is said that the snake-charmers drug themselves with daily doses of *Nux vomica*. There are cases recorded in this volume showing that as late as 1868 snakes have been employed for homicidal purposes. In olden times they were much employed in the East as the executioners in certain crimes.

Three cases of deaths from the stings of wasps and hornets and three from scorpions' stings are noticed; and Dr. Chevers has found (he does not say how or where) the following note: "In scorpion stings chloroform applied on lint to the part has proved a specific in sixteen cases.—H. G."

Decapitation (especially the removal of the heads of illicit lovers) seems to be still very common among the hill tribes. The injured husband usually, after having given his wife fair warning, cuts off the heads of the guilty pair, and so far from thinking that he has done anything wrong will not unfrequently tie the heads together and take them to the nearest magistrate. Dr. Chevers was told of a Kole (one of the up-country tribes)

who thinking that six old women had bewitched him, placed them in a row and cut off the heads of all, excepting the last, who ran away. In a chapter extending over twenty-five pages, the practice of "Human Sacrifice" is very fully considered. A very remarkable case that occurred in 1851, but was not tried till 1853, is described, in which one Joodhomonee living in the hills to the north-east of Chittagong, seized eleven Kookies who were sacrificed by torchlight at regular altars in the following manner. The victims, having been bound, six of his men armed with bill-hooks selected each his sacrifice, and having performed a religious ceremony struck off a head, their master and another salaaming at each successive sacrifice. Mr. Bowring, who tried the case, mentions in his report that the tribes inhabiting the Jaoni Mehals worship fourteen gods, and that "it has long been notorious that human sacrifices to these gods are not of uncommon occurrence." In Orissa, the practice of human sacrifice among the Khoonds residing in the hills is scarcely eradicated, notwithstanding the efforts of our government to suppress it. The victims of these sacrifices which are called *Meriahs*, must in every case be *bought*, otherwise they would not be acceptable offerings to the deities who are to be appeased. Until the time of sacrifice the persons intended for slaughter are well fed and kindly treated, just as was the case amongst the old Mexicans, and in the remarkable instance quoted by our author from Camerarius, who "tells us that, in a place hard by the city of Arles, in Provance, called La Rochette, it was the custom to buy three young men upon whom they bestowed good store of dainty victuals, and fattened them for a whole year, and after, at a day appointed, they cut their throats upon the altar, and with the blood sprinkled all the people standing devotedly round." This bloody ceremony, he adds, was abolished by St. Trophimas, one of the seventy-two disciples of Christ. As late as 1868 attempts were made to offer the *Meriah* sacrifice, but the parties concerned were arrested and punished. Buffaloes are now substituted for human victims. "Doubtless," says Dr. Chevers, "the crime is now less prevalent than it formerly was; but there are strong reasons for believing that there is scarcely a district in India in which human sacrifice is not still practised occasionally as a religious rite."

That human sacrifice was regularly practised in Lower Bengal within the last fifty years as a measure of *preventing famines* and of *checking pestilence*, is almost certain; and even as lately as in the famine of 1865-66 such sacrifices were, it is believed, had recourse to.

We extract the following remarkable and probably unique case from the chapter on "Fractures and Dislocations." In

December, 1868, one of the demonstrators of anatomy (a native) in the Calcutta Medical College, met with a calvarium (now in the College Museum) in which an iron headless nail, about an inch long, had penetrated the frontal bone on the mesial line, had passed completely through the longitudinal sinus, and had divided the layers of the *falx cerebri*, between which its point is visible; consequently its shaft, which was not corroded, stood in the mid-current of blood in the sinus, and seems, latterly, not to have caused any inconvenience. The edges of the hole in the frontal bone, which the butt end of the nail still occupies, are so rounded that it looks like an arterial foramen, and there was not the slightest trace of inflammatory change within.

The subject was an adult male, and it is conjectured that, as many huts are blown down in Bengal, the nail may have been driven in by an accident of this kind.

In a pathological point of view the chapter on "Ruptures or Wounds of Internal Organs" is the most valuable in the whole book. In a country where miasmatic poison is almost universally prevalent cases of rupture of the liver and of the spleen, from comparatively slight violence, are very prevalent. The nine years' Medical Reports to the Nizamut contain four cases of rupture of the liver due to kicks, blows, or to forcing the knee or foot into the right side. Cases of rupture of the spleen are certainly of frequent occurrence, but eleven years' reports gave only thirteen instances. Dr. K. McLeod has described *seven* cases, which he saw in a year, at Jessore, a very malarious district; in ten years Dr. Chevers met with only three instances, but he has found *sixteen* other cases reported, and he adds that "very many other cases might be collected by one specially engaged in investigating the subject." A slight blow, as with the open hand, or a simple push, has in many cases caused rupture of an enlarged and inflamed spleen.

A case of spontaneous rupture of this organ was observed by our author in a muscular young sailor, who was in hospital with intermittent fever, and Dr. Porter has recorded a case in which rupture seemed to have been caused by a tetanic spasm of the diaphragm and abdominal muscles. Death may occur from rupture of the spleen at any period, from a few minutes to several days. In the section on "Ruptures and Wounds of the Heart," the following remarkable case, narrated by Mr. White, then of Rangoon, is quoted. A soldier was wounded on the 14th of April, 1852. The ball entered the left axilla, taking an oblique direction to the cavity of the chest. The man did not die till the 24th of June, when, on examination, it appeared that the ball entered the chest between the third and fourth

ribs, and that its passage could be traced to the union of the pulmonary veins, but no further.

“On raising the heart, however, a hard and firmly impacted substance was felt at the apex, which proved to be the ball, in the left ventricle, at its most inferior part, crossed and recrossed by the *cordæ tendiniæ* and *columnæ carniæ*, which firmly secured it in its position. The heart was perfect in every part, and Mr. White considered that the ball must have perforated a pulmonary vein, and thus passed into the left auricle, and ultimately into the ventricle.” P. 474.

Injury to the testicles is a mode of assault so common in India as to require special notice. The danger arising from this form of attack was clearly recognised in the Jewish laws (see Deut. xxiv, v. 11), and the ancient Hindu law somewhat similarly observes that “if a Sooder should take hold of a Brahmin’s neck or testicles, the magistrate shall cut off both his hands.” The author describes five cases in which death ensued from squeezing the testes, and the Cossiah women seem to be specially well versed in this operation, for we learn of one thus murdering her husband, and another who thus frustrated an attempt at rape.

“Mutilation” forms the subject of a very interesting chapter. Beginning with “cutting off the hands and feet,” we find that this form of mutilation, although interdicted by our Government, still lingers in some of the native states. In 1868 we hear of its being practised in Cabul and Rajpootana, and in 1870 it was reported in the ‘Jubbulpore Chronicle’ for June that a thief was sentenced by the Maharaja of Gwalior to have his right hand cut off.

“Blinding,” as all our readers know, was a very common method throughout the East of treating conquered enemies and aspirants to the throne. At the present time this barbarism is confined to the lower classes, and cases of putting out the eyes by a heated iron, by a peg, or by gouging, not very unfrequently occur in the Indian courts; so also do cases of cutting off the ears, lips, and nose, the amputation of the nose being a common punishment of sexual crimes throughout India. Tippo Saheb seems to have been a ruthless destroyer of noses. He used to cut off the noses and ears of English prisoners who were caught in the attempt to escape. On one occasion, before Mangalore, he cut off these organs from the whole of a sepoy brigade; and in resentment of the resistance made by the inhabitants of Khis-tipoor, in Nepal, he cut off all the men’s noses.

Cases of mutilation of the generative organs are not very uncommon. In some instances women thus dismember their husbands or paramours for jealousy; in others, injured husbands

thus treat men intriguing with their wives ; but most frequently it occurs as a punishment for alleged wizards. In 1830, 150 inhabitants of Dewallee were tried for the murder of a wizard, whose penis they amputated, after killing him with sticks.

The manufacture of eunuchs is still carried on to a great extent in the states of Delhi, Lucknow, and Rajpootana. The operation is performed by eunuchs. The base of the genitals is encircled by a tight ligature, and the whole are swept away by a single stroke. Those who retain the penis are but little valued or trusted ; and a Mussulman who had lived for a long time among the eunuchs stated that he had only heard of one man having died out of fully a thousand operated on. In a deposition made by Khurrugjeet, a eunuch, regarding the murder of another eunuch, the deponent, after fully describing all the details of the operation, as performed upon himself at the age of sixteen, states that

“There are three sorts of eunuchs, viz., one born so, and the other two are made so ; one called Salundnee, and the other Badamee. The Salundnee Bijrahs (eunuchs) are chiefly made beyond the river Jumna, and the Badamee in Chundeyree. There is no fear of life in making the latter sort ; but there is in that of Salundnee, for they are made so by cutting literally the members from the root, while the Badamee are made by cutting off the half only.” P. 497.

Deaths from beating with various implements, and especially a shoe, a whip, or an office ruler, are by no means rare in India. Both the heavy leather shoes worn by Mussulmans and the common Hindustanee wooden shoes are dangerous weapons, and three or four blows with either form of shoe on the head may prove fatal. Dr. Chevers briefly notices five shoe-murders. Although not distinguished for pugilistic skill, the Bengali has a mode of bruising a prostrate foe with the elbow and fist, and sometimes with the knee also, which not unfrequently proves fatal. The practice of beating thieves and intriguers to death is said to be fearfully common both in Bengal and the north-west provinces. The Sikhs have a habit of making fictitious marks of beating upon their bodies, and then of making false charges of assault. By means of repeatedly rubbing the same place with a fragment of vitrified brick, they make a long red mark with so much verisimilitude that even an experienced person may be deceived unless he is aware of and suspects the trick.

Deaths from lightning are very frequent in India, no less than *sixty-two* persons having been thus killed in the Burdwan district alone in 1869. A native doctor, Baboo Odoy Chand Dutt, assistant surgeon at Pooree, describes the following fractures of the skull, in addition to other injuries, that were observed in the body of a youth of eighteen, who was killed by lightning in

1869. There was a longitudinal fracture commencing a little above the right frontal prominence, and passing to the back of the head; an oblique fracture commencing from above the left temporal bone backwards and inwards to the posterior end of the sagittal suture and then downwards and outwards, to meet the other fracture; and a fracture commencing from the first or longitudinal one over the right parietal prominence, passing inwards and forwards over the top of the skull, to the point of union of the coronal and sagittal sutures. Besides burns, there were several lacerated wounds on the abdomen, through one of which four feet of singed small intestine protruded. On opening the skull the right side of the brain was found to be covered with a thin layer of blood.

According to Dr. Oldham, the well-known geologist, aerolites have been known to produce injuries, which have, however, not proved fatal, by their fall.

The subject of "Torture" is treated at great length, and the chapter devoted to the subject reveals a most appalling condition of society. The different modes of torture are so numerous that we cannot even mention the whole of them. There are separate sections on torture by heat and by cold; by the insertion of red pepper into the eyes, anus, vagina, urethra, &c.; by stinging nettles and thorns; by the introduction of a ruler, stick, &c., into the vagina or rectum; by compression of the chest; by compression and pricking of the testicles; by binding the hands and body with tight cords; by inserting the body into a warm bull's hide or sheepskin; by peculiar forms of stocks; by placing stinging and gnawing insects over the navel, eyes, &c.; by sticking pins or splinters of wood under the nails; by holding the victim under water; by pulling the hair on both sides or suspending by the moustache; and by modes of inflicting degradation and mental suffering.

Torture is blended with the customs of all sects and classes of natives throughout India. "The poor," says Dr. Chevers, "practise torture on each other; robbers on their victims and *vice versa*: masters upon their servants; zemindars upon their ryots; schoolmasters upon their pupils; husbands upon their wives; and even parents upon their children." Torture by heat in some form or other is often applied for the most trivial causes. For example, a native female burnt the lips of her sister-in-law, a girl aged twelve, with a pair of heated tongs, "solely because the latter had, the day previously, eaten a portion of the other's dinner;" a man branded a young boy with a hot sickle "for an act of ordinary carelessness;" a railway gate-keeper having interrupted six men in taking away some sleepers was seized and held over a fire till he fainted; a man having lost

some small article from his house, "proceeded as a matter of routine to dip the hands of his three wives into boiling cow-dung, to induce them to confess that they had taken it;" and similar cases could be quoted to almost any extent.

One of the most common forms of torture in Madras is the *Anundal*, or tying a man down in a bent position with a rope passed over his neck and under his toes; the head being brought down to the knees.

As illustrations of the last form of torture mentioned in our list, we may mention having the hair of the head shaved off with the urine of an ass; compelling respectable people to clean out privies and thus lose caste; violating females in the presence of their male relations; making the women of a family walk naked through bazaars, &c.

In the chapter on "Hanging," we meet with a new test for deciding whether or not a body has been hung up after death. In suspension *during life* the flow of saliva out of the mouth down the chin and straight down the chest may be always observed. "The appearance is unmistakable and invariable, and could not occur in a body suspended after death."

In the following chapter, treating of "Suffocation," Dr. Chevers gives us five cases of death occurring from the impaction of a living fish in the air-passages, three of which occurred in India during the last few years. In one of these cases a large cat-fish jumped into the mouth of a Madras Bheesee while bathing.

"The operation of œsophagotomy was commenced and abandoned. A piece of cane was made into a probang and attempts were made to press the fish downwards into the œsophagus. It did pass downwards, when the patient at once ceased to breathe, gave a convulsive struggle, and died to all appearance. The trachea was immediately opened, and respiration was restored. In the course of the night the man coughed up the fish, the fins having been softened by decomposition."

Dr. White, who reported this case in 1863, states that—

"This is by no means an uncommon accident in India. Natives bathing and swimming, which they always do, with their mouths wide open, in tanks that abound in fish, are not unfrequently brought to hospital dying from suffocation and alarm with a large cat-fish impacted in the fauces."

The four other cases seem to have terminated fatally.

From the chapter on "Drowning" we learn the prevalence of the habit of destroying children by throwing them into wells, the mother often being the murderer, and the cause exciting the crime being apparently slight. A mother who dropped her

baby into a well, pleaded "not having milk for her infant," and this being (we think properly) taken as an extenuating circumstance, she was only imprisoned for life. A grandmother drowned two of her grandchildren "because they asked her for some food which she had with her." A mother drowned her two children because she "felt aggrieved with her sister-in-law;" and often no cause whatever is assigned. The cases in which the mother, if she is ill-used or offended, at the same time attempts to drown herself, are not rare. Two native women, A. and B., having intrigued with each other's husband, had a quarrel, and A. threw herself and her infant into a well. She was taken out alive, but the child perished. The next day B. (probably from a feeling of etiquette) threw herself into a well, but was brought up alive. A woman after a dispute about a trifling debt jumped into a well with her infant. She was extricated alive, but the child was drowned. Many similar cases might be quoted, in which the most common result is that the female is rescued and the child perishes.

Adults as well as children are not unfrequently murdered by drowning in wells; and there is a popular idea that this is the ultimate fate of bold young lovers who allow themselves to be introduced, in meat doolies, into seraglios. In a long footnote Dr. Chevers has collected a number of cases in which persons thrown into wells were preserved almost unhurt. The following are perhaps the most remarkable instances: "A woman of eighty travelling with two men who carried her property was told to look into a brick well where she would see the deity Shee. She did so and was thrown in, but fortunately clung to a brick and supported herself in the water which was eighteen feet deep." In the same year (1852), "a feeble woman, stating that she was aged eighty or a hundred years, was robbed of her ornaments and thrown into a well, but she supported herself by clinging to the branch of a tree." The old proverb, "fore-warned is fore-armed," was curiously illustrated in the case of Jung Bahadour, who knowing the custom which the Nepaulese have of dropping people into wells, practised the art of falling into them with impunity.

In connection with the subject of drowning there are some valuable remarks on the time at which submerged bodies rise in India. Dr. Woodford states that the *shortest time* in which he has known a body in that condition to rise in that country in the hottest season has been twenty-four hours; that is to say, when the body has been entirely submerged in deep water. In shallow water, as at the edge of a sloping tank, a body may rise much sooner. In January, 1867, from thirty to fifty persons were drowned in the Hooghly from an accident to the

steamer "Calcutta," at 6.30 p.m.; the thermometer standing at 75°. Eight bodies were recovered, and Dr. Chevers noted the different periods at which they came to the surface. Three natives were submerged 87 hours; four European women were submerged 95, 112, 136, and 181 hours; and a male European rose after 95 hours. He attributes these differences of time mainly to the differences of dress which would modify the period of decomposition.

Until a few years ago the practice of "Burying Lepers alive" seems to have been common in Bengal, and cases still occur in the north-west provinces. As late as 1868 it was ascertained that the custom was then prevalent in some of the Rajpootana States, and was not extinct in Cashmere. Dr. Chevers has no faith in the cases that have been reported of Faqueers and others allowing themselves to be isolated for months from the external world without food, drink, or fresh air; and quotes two instances, one of which occurred in 1868, and the other subsequently, in which the experiment was tried with a fatal result.

The chapter on "Rape" abounds in most horrible and revolting details, but they are such as must necessarily have a place in a volume of this kind. In relation to puberty, Dr. Chuckerbutty states that he knows an unquestionable case in which a girl became a mother at *ten*, and adds that girls are frequently mothers at twelve and thirteen. Dr. Green, of Dacca, performed craniotomy in a case where the mother was twelve. Dr. Hutchinson sent the author "two cases in which European girls born and brought up in India menstruated so early that they might have been mothers when *eleven years and seven months* old." According to Mahomedan law, "the earliest period of puberty with respect to a girl is nine years," and the practice founded on the law is "that a man has intercourse with his wife only when she is about to menstruate for the first time." As a medico-legal curiosity we may notice a case in which "a boy only *ten* years old was convicted of rape on a girl three years of age." The punishment was imprisonment for one year.

In sixty-six trials for rape from the printed Reports of the Nizamut the ages of the females are stated as follows: Four years, 1; six, 2; from six to seven, 6; from seven to eight, 3; nine, 3; from nine to ten, 12; from ten to eleven, 3; from twelve to thirteen, 7; fourteen, 1; fifteen, 1; from fifteen to sixteen, 3; adult women, 19; widows, 5. It will be observed that in nearly half the cases the females were under the age of twelve.

In this country when several men have successively ravished

a woman, the unfortunate victim is left in a state of intense nervous exhaustion. When a woman submits voluntarily to the embraces of a considerable number of men, as in the reputed case of Messalina, the conditions are of course extremely difficult. Yet we confess that we feel a difficulty in accepting the astounding "attested statement" made by Mr. Lawson "of what he and others have observed" among the Marquesan women. "He mentions one case in which he heard a parcel of boys next morning, count over and *name* 103 men who during the night had intercourse with *one* woman."

In consequence of the early age at which marriages are contracted in India, many cases of young females of from eight to eleven being literally raped to death by their husbands are on record. From various facts collected by Dr. Chevers, it is evident that the ancient law-givers of India, "in providing for the early marriages of females, were careful to protect them against too early cohabitation; and the practice of sending immature children to their husbands' houses, which at present maintains, is evidently as much an infraction of the law of the land as it is a violation of the law of nature."

The chapters on "Abortion," "Delivery," and "Infanticide," extend over more than sixty pages, from which, if space permitted, we should make several extracts.

The concluding chapter is devoted to the subject of "Insanity," and contains, *inter alia*, the most complete history of *Running Amok* (or *A Muck*) that we have ever met with. We shall finish this article with a notice of "a set of cases in which a morbid appetite for alcoholic drinks occurs as a symptom of cerebral disorder." Dr. Chevers tells us that he has had under his own treatment two cases in which this was distinctly observed, and he knows that these are not solitary instances. Every one must agree with the learned author, that "the very important medico-legal question which they involve should not be overlooked."

VI.—Sanitary Reports.¹

WE suppose it will scarcely be gainsaid that the reports

¹ 1. *Twelfth Report of the Medical Officer of the Privy Council.* With Appendix. 1869.

2. *Second Annual Report of the State Board of Health of Massachusetts.* January, 1871.

3. *Report on the Drainage and Conservancy of Calcutta.* By DAVID B. SMITH, M.D., Sanitary Commissioner for Bengal. 1869,

issued by the medical department of Her Majesty's Privy Council constitute a model upon which such reports should be framed, and may be held up to the world as an example of the sort of work which a central government in any country may fairly be expected to do for the general benefit of the community. There are two kinds of investigation which the department undertakes; the first is that which of course no government which claims to care for the public health can well avoid, namely, the investigation of serious outbreaks of disease, to which it adds the enforcement of the law as it stands or its amendment where defective, as it relates to the prevention of disease. The second is apparently of less immediate practical importance, and therefore commends itself less directly to public attention. We allude to the purely scientific inquiries carried on by experts under the direction of the able medical officer who presides over the department. Abstruse in their nature, difficult to prosecute, and making great demands upon the time, such inquiries would probably never be made at all were they left to the spontaneous efforts of men who could only devote to them the little leisure that might remain after the performance of the ordinary professional duties of the day. It is pleasing to observe how the example set by our government has been followed in the United States of America. In a former and recent article we gave an instance of this as exhibited in the Reports of the Metropolitan Board of Health of New York. We have now also to notice with commendation the Second Annual Report of the State Board of Health of Massachusetts, as combining the same two features.

The special diseases of the year 1869 considered by Mr. Simon in his Twelfth Annual Report on Relapsing Fever and Scarlatina. The former was, as will be recollected, a comparatively mild outbreak of a disease which only visits this country at rare intervals. In the month of October 130 cases were admitted into the London Fever Hospital, in November 259, and in December 315. By the beginning of the new year relapsing fever had attained its chief extension over the poorer parts of London. In January the admissions into the London Fever Hospital were 258, in February only 153 cases were received into this and the Hampstead Hospital together, and in March only 140. As usual, the mortality was low; of the 769 cases admitted into the London Fever Hospital in 1869, only 17 died, and these few deaths occurred chiefly among persons previously weakened by age or disease. The epidemic of scarlatina was a very much more serious affair, since in London alone by the end of the year as many as 5803 lives were sacrificed by it. Mr. Simon justly remarks, that at the present time, with scientific

knowledge limited as it is, and with our very imperfect administrative resources, any one who is responsible for advising on the requirements of the public health must feel extreme difficulty, and indeed almost humiliation, in having to advise about scarlatina :

"It is profusely and to a certain extent uncontrollably contagious; uncontrollably in so far as science cannot yet offer against it any such personal protection as vaccination confers against smallpox; uncontrollably, again, in so far as, in order to spread, it does not, like typhoid fever and cholera, depend or mainly depend on conditions which moderate sanitary care removes; uncontrollably further, in so far as its contagion is of most persistent activity, and remains in force for indefinite periods of time, in clothing, bed furniture, and other objects which give it a resting place." . . . "At present we have not any other known power of dealing preventively with the disease than such as consists in intercepting all contagious communication between the infected and the non-infected parts of the population. Thoroughly to isolate the sick from intercourse with susceptible persons, and thoroughly to trap and exterminate all contagion which the bodies of the sick evolve, are the preventive feats which have yet to be accomplished. The difficulties of the task are extreme. Often they are not successfully encountered, even in wealthy and well-ordered establishments, with every material and educational advantage; and no one who is conversant with the domestic circumstances and habits of the great masses of our population can expect that among these millions, particularly in the poorer and more crowded dwellings of the labouring classes, the spreading of scarlatina will be very effectually resisted by such measures as alone are yet available." (p. 13).

Still Mr. Simon holds out a hope that in time some other mode of resisting this unmanageable disease will be discovered. He says:

"I cannot express in adequate terms how very great an importance ought, in my opinion, to be attached to the sorts of scientific research by which such results may be realised. Considering, on the one hand, what Jenner's discovery in relation to one such disease has done for the whole human race, what addition to collective human strength it represents, and what mitigation of human suffering; and considering, on the other hand, the immeasurable ravages of the remaining unconquered contagia, I cannot conceive any scientific problems worthy even to be compared in importance with such as are here referred to" (p. 14).

The papers of Dr. Sanderson "On the intimate Pathology of Contagion," and of Dr. Thudichum "On Chemical Identification of Disease," which form part of the appendix to this volume, are the contributions of the department for 1870 to this

kind of inquiry. The consideration of the latter of these papers will best be taken up, either when the investigation is more advanced, or in connection with similar researches by other observers. The former alone will briefly occupy us here.

Dr. Sanderson is obviously right when he says that the question of the mode of existence of contagious matter lies at the foundation of all scientific inquiry as to the means of obviating or counteracting contagion. In the report we have before us now, he professes to proceed no further than the consideration of certain points which, being of a fundamental nature, must necessarily be first disposed of. These points are the physical properties of contagia, and the presence or absence in infecting liquids of organic forms possessed of those distinguishing characters which mark off living from dead substances.

The first step in the inquiry was to show that the "infecting matter" is insoluble in water or watery liquids, and indiffusible, and the facts and observations on which Dr. Sanderson relies for this purpose are those announced by Dr. Beale, M. Chauveau, and himself. Starting from vaccine lymph as a convenient matter for experimental research, we have first of all the observation of Dr. Beale, of the existence in it of transparent hyaline particles of extreme minuteness, which he regards as masses of living or germinal matter. In fact, there are three elements in vaccine, "leucocytes," which do not differ from the corpuscles observed in the liquid of a blister when first becoming opalescent, minute particles not exceeding $\frac{1}{100000}$ th inch in diameter, and a clear liquid in which they both float. While determining which of these three elements was the infecting one, M. Chauveau found that the leucocytes were readily separated by the addition of ten volumes of water, when they subsided. He found that they were entirely inactive when employed for vaccination. The question, then, lay between the granulations and the serum, and in order to obtain their separation the one from the other, he had recourse to diffusion with water brought into contact with the vaccine without the intervention of membrane: under these circumstances all the soluble constituents, albumen, &c., passed into the water, leaving the particles at the bottom of the vessel. The use of the upper strata of the liquid (free from particles) for vaccination produced no effect upon children or heifers, while the ordinary results followed the use of the undiluted lymph. This experiment was repeated by Dr. Sanderson, who improved upon the manipulation by the use of a more delicate apparatus for diffusion, which not only enabled him to avoid disturbing the surface of the vaccine lymph employed, but to employ definite and varying weights

of vaccine and water. His results were entirely confirmatory of those by M. Chauveau.

Dr. Sanderson next refers to Chauveau's experiments with smallpox virus, performed upon the ox, on which it produces a reddish papule of a specific character. In this case, also, he found that the diffusate was as inactive as when vaccine matter had been used in a similar manner. But perhaps even more convincing are Chauveau's experiments with the virus of sheep-pox. The appropriateness of this virus for experiment lies in its being, like smallpox in the human subject, an eruptive disease, and, like it, in its being communicable through the air at great distances. The results of his experiments with this virus were communicated to the Academy in 1868, and they show even more conclusively than as regards vaccine that the virus is neither soluble nor diffusible, and, consequently, that when it is communicated from one animal to another, through the air, the fact signifies simply that the contagious particles are wafted by currents from the diseased organs of the one to the other. He experimented (1) by diffusion, when the diffused liquid was found to be absolutely inactive; and (2) by dilution. Operating on the latter method, he found that punctures with the contagious liquid of sheep-pox, when diluted with 500 times its weight of water, yielded thirteen enormous pustules to twenty-one punctures, and that, even when diluted with 10,000 times its weight of water, a single pustule resulted from twenty punctures; and it became the starting-point of a general eruption. The local effect produced (provided it was produced at all) was always the same. The difference was found to lie entirely in the numerical relation of successes to failures. If successful, inoculation with a liquid containing a myriadth of infecting juice communicates the disease as completely as the pure liquid itself. Dr. Sanderson upon this remarks:

"This fact is not only perfectly consistent with the notion that contagium is particulate, but is in itself a necessary consequence of its being so. If, on the other hand, contagium were soluble, it could not be explained, for in that case each of the 10,000 drops in which the one drop had been dissolved would be equally active. Assuming it to be particulate, it follows that the myriad particles which were before distributed in one drop are scattered through 10,000 drops. And inasmuch as there is nothing except the influence of currents to ensure the equal distribution of the particles, it is clear that in some regions of the liquid the distance from each other will be greater, in others less. Consequently, when a trace of the liquid thus feebly impregnated with contagium is taken on the point of the lancet, the chance that the little drop will or will not contain particles may be stated numerically by the fraction which expresses the degree of dilution" (p. 241).

A similar explanation applies to all those diseases which are contagious at a distance. Assuming the contagion to be insoluble and particulate, the question of mediate contagion, like that of direct contagion, must be one of chance—the chance of meeting in the medium with the contagious particles.

With regard to the nature of the contagium particles, Dr. Sanderson accepts the doctrine that they are organised beings, and that their powers of producing disease are due to their organic development, as the only one which affords a satisfactory explanation of the facts of infection; and in particular of facts which tend to show that the particles rapidly reproduce themselves within the body of the infected individual, while out of the body they are capable of resisting, for very long periods, the influence of conditions which, if not restrained by organic action, would produce chemical decomposition. If the contagium consists of living particles, naturally their analogue would be sought for among the smallest plants or animals. The conditions of their origin and growth point to the fungi. The most minute of known organisms are the spheroidal corpuscles known as “microzymes,” and it is by an experimental investigation of the organoleptic properties of these bodies that Dr. Sanderson thinks we may most reasonably hope to arrive at a solution of the problem of contagion.

We have dwelt at some length upon these portions of Mr. Simon’s volume on account of their supreme importance in connection with a class of maladies which destroy annually a full fourth of the population of Great Britain, namely, diseases which take their origin in the admission into the economy of a material which reproduces itself largely therein, and, being given off therefrom, spreads disease to other individuals.

The second subject treated of by Mr. Simon is the supervision exercised by his department over local nuisance jurisdiction and water supplies; and in relation to this subject, and especially the avoidance of excremental nuisance, we are supplied with two most careful papers, the one by Dr. Buchanan upon the dry earth system of dealing with excrement, and the other by Dr. Buchanan and Mr. J. N. Radcliffe, jointly, on the construction of midden closets, &c., in several of the northern towns in the kingdom. This latter report is rendered doubly valuable by the numerous plans and sections which illustrate it. Indeed, these two papers form a repertorium of facts and an exposition of contrivances, with criticisms upon them, such as leave no excuse in future to local authorities, who have but the will to enter upon the work of abolition of excremental pollution.

Upon this follows the customary report upon public vaccina-

tion, in which the writer comments upon the subject of "animal vaccination" as a source of lymph supply, basing his remarks upon a strikingly unfavorable report drawn up by Dr. Seaton. Mr. Simon considers that the disadvantages of the system, as at present understood, are its various peculiar liabilities to failure, and that they are such as entirely to forbid its adoption in place of our own, unless on independent grounds our own already deserves to be condemned. We think that here Mr. Simon goes beyond the mark.

We are not aware of any British authority who has even suggested such a thing as displacing the practice of arm-to-arm vaccination in favour of vaccination from the heifer. The utmost that has been advocated has been that an opportunity should be afforded to practitioners to renew a deteriorated stock of lymph from the heifer; and that timid persons, who dread the invaccination of syphilis, should be enabled to obtain vaccination for their children under circumstances prohibitory of such an accident. But Mr. Simon is one of those who, up to a late period, has denied the possibility of such an occurrence, except under conditions of great carelessness on the part of the vaccinator. So in this matter he is quite consistent. Whether he is right, or whether Dr. Seaton has not been led away to overstate his case against heifer vaccination, is another question. We shall be prepared to hear that the recent investigations of Mr. John Hutchinson into some alleged instances of syphilitic invaccination, which came accidentally under his notice, have altered the opinions of a good many people upon this subject.

The two final sections of Mr. Simon's report have reference, the one to the state of the law as it affects the practice of pharmacy in Great Britain, in which he deplors the inoperative character of that section of the Adulteration of Food Act which relates to the adulteration of drugs; and the other to the constitution of the medical profession and the working of the Medical Act, 1858.

The Second Annual Report of the State Board of Health of Massachusetts is a highly creditable production. During the year 1870 several special investigations, of which full reports are appended, were made under the direction of the Board. The subjects were lead poisoning, trichiniasis in Massachusetts, the health of towns, charbon or malignant vesicle, typhoid fever, the houses of the people, alcoholic drinks, the ventilation of school houses, the impurities of a water supply known as the "mystic pond," the impurities of the air, the health of minors employed in the manufacture of cotton wool, silk, flax, and jute, and the effects of the use of sewing machines on the

health. We desire especially to direct attention to that section which relates to alcoholic drinks, which contains a vast amount of valuable information as to their use and abuse in all parts of the world, obtained from the American ministers in foreign courts and the consuls of all the principal ports on the globe. We are acquainted with no returns so complete.

The report of Dr. David B. Smith upon the drainage and conservancy of Calcutta is a document which cannot be read without a painful feeling. It is a prolonged record of chronic apathy and administrative inefficiency of which the government of this important city has reasons to be thoroughly ashamed. It will be sufficient to show this if we quote a few passages out of the summary with which this report closes.

"1. It is beyond^{*} contradiction that the present condition of Calcutta is indescribably filthy, and a reasonable cause of alarm to the sanitarian.

"2. For sanitary mal-administration Calcutta will fairly compete with the most neglected cities.

"4. Scarcely any possible un-hygienic condition is here absent.

"6. In this respect (conservancy) almost everything that is done ought not to be done, and *vice versâ*.

"11. Two hundred people lately died of cholera in a fortnight, within the area of a square mile in the heart of Calcutta; and the fact can scarcely be said to have attracted general public attention.

"21. The 'compound-privies' of Calcutta are more obnoxious and revolting than anything else of the sort I have ever seen. Some of the Tolâh Mehter Depôts are almost unapproachable; and the 'private privies' scattered by hundreds over the town, consisting of small patches of ground soaked with ordure and only surrounded by low screens of matting, are surrounded by an atmosphere overpoweringly fetid.

"33. A far greater evil than the last (making a railway embankment just outside the city of the sweepings of Calcutta) is the continued wilful pollution of the River Hooghly, effected by casting the night soil of Calcutta into the stream, immediately above all the shipping of the port.

"138. . . . Prosecutions for conservancy offences have become a subject of derision amongst natives."

How much longer is all this to continue?

VII.—Capillary Emboli and Infarctus.¹

TEN years have now passed since, in our July number for 1861, we devoted an article to a review of the subject of emboli, based chiefly on the important book of Cohn. The subject was then a new one, and a considerable portion of that article was occupied with an historical survey.

Two forms of emboli were then distinguished: 1, those of the arteries; 2, those of the capillaries.²

In the period which has elapsed since the appearance of the Review just referred to, the theory of embolism has made important advances, but the progress of science has hardly been uniform in the two divisions of the subject. As regards arterial embolism, the opinions which prevailed ten years ago have received little but confirmation. But the doctrine of capillary embolism has in the course of time been greatly expanded, and has received many valuable additions.

We have, therefore, been glad to avail ourselves of the opportunity afforded us by the issue of a second edition of Feltz's book to review the present position of this branch of our subject.

It was formerly supposed that capillary emboli were important only on account of the secondary processes to which they give rise, and not (like arterial emboli) in consequence of their directly impeding the circulation. It is well known that the impaction of a plug in the main trunk of the pulmonary artery is a frequent cause of sudden death, and that embolism of a large cerebral artery may give rise to hemiplegia; but it has generally been believed that the effect of capillary embolism is rather to induce changes in the surrounding tissues, and that the only

¹ 1. *Traité Clinique et Expérimentale des Embolies Capillaires.* Par V. FELTZ, Lauréat de l'Institut, Professeur Agrégé à la Faculté de Médecine de Strasbourg, Directeur des Autopsies, &c. &c. Deuxième Édition. Strasbourg, 1870.

2. *Etude Physiologique et Pathologique sur les Infarctus Viscéraux.* Par — LEEUWRE. Thèse de Paris, No. 195. 1867.

3. *Experimentelle Untersuchungen zur Physiologie und Pathologie der Embolie, Transfusion, und Blutmenge.* Von Dr. P. L. PANUM, Prof. in Kiel. Berlin, 1864.

4. *Pyæmia.* By JOHN SYER BRISTOWE, M.D. (in 'A System of Medicine,' edited by Dr. RUSSELL REYNOLDS, vol. i, 1866).

5. *Thrombose und Embolie. Gefässentzündung und septische Infection.* Von RUDOLF VIRCHOW (Gesammelte Abhandlungen zur wissenschaftlichen Medicin). Frankfurt, 1856.

² It ought to be mentioned that the difference between these two forms of emboli is not quite that which would be implied from their respective names. The term "arterial emboli" is used only for those which occlude the larger arteries, so as to cut off an important part of the circulation; those penetrating to the smaller arterial branches are in practice always classed as "capillary emboli."

clinical phenomena resulting from capillary embolism are such as arise from these tissue changes, and from the blood-infection which is apt to accompany them.

Now, the earlier part of Feltz's work is devoted to the attempt to prove that this belief is erroneous; that capillary embolism itself may give rise to sudden death, and to the other mechanical effects which are familiarly known to result from arterial plugging. We, therefore, propose in the first place to lay before our readers as briefly as possible the evidence by which he endeavours to establish this position. And as throughout the book clinical observations and experimental investigations are placed side by side in each chapter, we will begin by relating in a condensed form the principal case which Feltz adduces in illustration.

M. R—, æt. 28, was admitted into the Medical Clinique of Strasbourg, in July, 1865, with swelling of the left lower limb, due to obliteration of the femoral and saphenous veins. She had been confined six weeks before.

After her admission she improved considerably, and a rapid recovery was looked for, when, on August 6th, she was suddenly attacked by symptoms of commencing asphyxia, becoming livid and panting for breath. Auscultation yielded no explanation of these symptoms; the vesicular murmur was audible over all parts of the lungs. The heart was beating violently, but without any morbid bruit.

Dr. Feltz was inclined to attribute the attack to embolism, either of the right heart or of the pulmonary artery. However, next morning, the alarming symptoms subsided. The idea of embolism was, therefore, given up. Some days afterwards the asphyxia returned, and the patient succumbed to it.

At the post-mortem examination the femoral vein was found to be completely obliterated. The clot projected upwards into the common iliac vein; its end seemed to have been torn off, and its interior was hollow, admitting the point of a probe. It was of a dirty yellow colour, and was easily separated from the wall of the vein. It contained a quasi-purulent liquid.

On microscopical examination it was evident that in the softened central part of the clot the elements of the blood had undergone fatty disintegration, the fibrin being converted into a molecular material. When the quasi-purulent liquid was mixed with water there were precipitated a quantity of small fragments of about the size of pins' heads, composed of granular portions of fibrin and of white blood-cells.

The dissection was now carried upwards, in the expectation that the missing extremity of the clot from the iliac vein would be discovered, impacted either in some one of the large branches of the pulmonary artery, or in one of the cavities of the right heart itself. But the clots which occupied the right ventricle and auricle were found to be recent and to contain no trace of *débris* of the thrombus;

and the trunk and main subdivisions of the pulmonary artery presented only soft fresh coagula, of which the consistence was very different from that of the clot in the femoral vein. It was only when nearly all the ramifications of the artery visible to the naked eye were opened up that they were found to contain red coagula, which, when treated with water, yielded a deposit of yellowish grains. These grains were discovered on microscopical examination to be composed of fibrin in a state of fatty degeneration, precisely similar to that of the coagulum in the femoral vein. When the red coagula were withdrawn from the apparent terminations of the pulmonary artery, there were also obtained certain whitish threads which appeared to be of no recent formation. The lungs were very red, gorged with blood, their state, in fact, resembling that found in the bodies of those who have been hanged.

The consideration of this case led Feltz to perform some experiments with a view to determine whether rapid death from asphyxia could be produced in the lower animals by the injection of pulverulent matters into the veins. By way of illustration we may take his first experiment:

On the 3rd of June, 1866, he injected into the left jugular vein of a full-grown rabbit one cubic centimetre of powdered charcoal, suspended in six c. c. of water. Before the injection was completed, the animal showed all the signs of asphyxia, and it died while the canula was being withdrawn. The lungs were found to be gorged with blood. The right heart and pulmonary artery contained some of the injected material, and with the microscope it could be easily recognised in the small pulmonary vessels; indeed, in some parts the position of the capillaries was indicated by black lines. No trace of the charcoal could be found in the pulmonary veins, nor in the systemic arterial channels.

The experiment was subsequently repeated thirteen times with different substances, and always with the same result. Twice powdered charcoal was employed, three times fibrin which had been dried and reduced to a pulverulent state, in two instances three c. c. of pus formed the material injected, once five c. c. of a liquid holding cancerous elements in suspension, three times disintegrated tuberculous material (from vomicae), once three c. c. of olive oil. Microscopical examination showed that the separate granules of the powders employed were in each instance scarcely larger than blood-corpuscles or pus-cells. But when suspended in liquid, they collected together and formed masses of greater or less size. Two guinea-pigs were subjected to similar experiments, and died in the same way as the rabbits. In addition to the other symptoms of asphyxia, it may perhaps be well to note that the temperature of the rectum fell rapidly.

These experiments are conclusive as to the *possibility* of producing sudden death in small animals by the introduction of pulverulent substances into the systemic venous circulation,

And, as is remarked by Feltz, an analogous fact may be found in the mode of death after introduction of air into the veins. Michel demonstrated that in the frog air kills by stopping the circulation through the pulmonary capillaries, the little columns of air which are formed oscillating to and fro in these vessels under the influence of the cardiac beats. Feltz has repeated Michel's experiment on the rabbit, and has obtained similar results.

But in applying to human pathology the fact that rabbits can be killed almost instantaneously by the injection of pulverulent substances into the jugular vein, it is important to bear in mind that the production of this effect depends entirely on the quantity that reaches the pulmonary vessels. In a later series of experiments Feltz found that the injection of pulverized fibrin into the *femoral* vein produced merely a more or less severe dyspnœa, which either passed off entirely, or terminated fatally only after a considerable interval. A large part of the injected matter, collected into masses and mixed with coagula, was found within the venous trunks, especially near their valves.

From these considerations it is, we think, evident that death can very rarely occur in the human subject as the direct result of capillary embolism of the pulmonary vessels. It must very seldom happen that a venous clot should give rise to sufficient detritus to plug a very considerable part of the minute arteries and capillaries of the lungs, and that this detritus should be poured into the blood current in such a way as to be diffused evenly, and to escape the tendency to aggregation which showed itself so plainly in Feltz's later experiments.

The case above referred to from the Medical Clinique of Strasbourg is not, indeed, entirely above criticism. Nothing is said about the condition of the bronchial tubes; and yet we think that those who have had most experience in regard to practical morbid anatomy will be most ready to admit that the discovery of capillary bronchitis is often a matter of very great difficulty, even when that disease has undoubtedly been the cause of death. Nor does the present state of science appear to warrant the expectation that a visible cause is invariably to be found in the dead body, which shall account for the existence of dyspnœa during life. It is, however, the object of these researches to diminish the number of unexplained deaths; and we are quite of opinion that Feltz's case makes it incumbent on future observers to examine carefully the minutest ramifications of the pulmonary artery in all cases of suspected embolism, and not to rest contented with an inspection of the main trunks.

The objections at which we have just hinted apply likewise with great force, as it seems to us, to Feltz's second case,

A man, æt. 31, is admitted into the Clinique of Professor Schützenberger, of Strasbourg. He suffers from gastric symptoms of three weeks' duration; three days later he is delirious. Two days after this he has two severe attacks of dyspnœa, in the second of which he dies. The right ventricle is found to contain a greyish-white coagulum, from which proceed a large number of filaments, forming a shaggy mass. In the small ramifications of the pulmonary artery fine threads of clot are discovered, which evidently once formed part of the filamentous mass just described. Thirty of these are removed by one of the assistants.

That these thread-like clots were emboli may be admitted; but we cannot help thinking that to account for the dyspnœa that had been observed during life a much larger number of them would have been required than appears to have been found.

The third case is that of a woman, æt. 59, who had a cancer of the thyroid body, which grew into the jugular vein. She had repeated attacks of dyspnœa, in one of which she died. In two of the principal branches of the pulmonary artery there were tumours which incompletely obliterated the calibre of the vessel. In the ramifications of the third and fourth order there were also masses of cancerous tissue adherent to the arterial coats; while the very smallest divisions contained loose carcinomatous fragments, unconnected with the vascular walls, and some of them embedded in minute filiform clots.

This case is a very interesting example of the embolic propagation of cancer; but there appears to us to be no proof whatever that it affords any illustration of the effect of *capillary* embolism in producing dyspnœa, or in giving rise directly to the fatal termination of the case. The same may be said of the next case (quoted from Weber, of Heidelberg), in which the branches of the pulmonary artery of the third order, as well as still finer twigs, contained masses of an enchondroma, which had become detached from a growth extending into the iliac veins.

Lastly, two cases are given in which death was caused by embolism of the main trunk of the pulmonary artery, but in which there had been several previous attacks of dyspnœa. In the various branches of the pulmonary artery were found a large number of smaller coagula, which (in one case, at any rate) were evidently of older date. To these are ascribed the earlier paroxysms, and doubtless with justice; but they were in no sense *capillary* emboli.

The next topic discussed by Feltz is the occurrence of embolism of the smaller branches of the pulmonary artery,

secondary to thrombosis arising in the veins of parts burnt or frozen:—

A girl, æt. 21, was admitted into Prof. Michel's clinique in Strasbourg, in Jan., 1867, with her feet intensely frost-bitten. She had for a week been exposed night and day to the inclemencies of a very severe winter. On her admission the feet were of a bluish-brown colour, the toes nearly black. Next day her respiration was much embarrassed, and on the following day it was stertorous. Rhonchus was heard, mixed with the vesicular murmur, over the trachea and bronchial tubes. These symptoms afterwards subsided in a marked degree; but tetanus set in, of which she died on the ninth day after her admission.

On post-mortem examination it was found that many of the smaller branches of the pulmonary artery contained clots which filled their cavities. These clots were not homogeneous, but were composed of small firmer fragments, embedded in comparatively soft red coagula. Opposite the capillaries which were thus plugged the lungs presented red indurated nodules (infarctus), yielding blood when cut into. These nodules existed especially in the lower lobes. The veins on the dorsal surfaces of the feet were likewise full of whitish clots, and in the saphenous veins these extended as far as the middle of the legs. The deeper veins, on the other hand, were permeable, even in the soles. The superficial parts of the feet were gangrenous.

Microscopically, the white portions of the clots were found to contain a large quantity of free fat in the form of minute drops, numerous red blood-corpuscles in various stages of molecular decomposition, some normal white blood-cells, and lastly certain fusiform cells, no doubt due to the desquamation of the vascular epithelium. Such cells were even observed in considerable numbers coating the surface of the clots removed from the finest capillaries of the lungs. The blood contained in the frozen toes was composed of red blood-corpuscles much smaller than natural, and deformed.

We are quite ready to grant that in this case embolism took place from the veins of the frozen parts into the small vessels of the lungs, a condition which was diagnosed two days after the patient's admission. But we must confess that some doubt still remains in our mind as to whether these minute embolisms had much to do with the severe dyspnœa from which the girl suffered, and which would seem to have been at least as likely to have been caused by bronchitis, a very natural result of the exposure she had undergone. It seems to us an important omission that in the report of the post-mortem examination nothing should be said as to the state of the smaller air passages.

As the pulmonary symptoms had subsided before death (which was due to tetanus), the case is described by Feltz as indicating the possibility of the cure of capillary embolism of the pulmonary

artery; a mode of termination which has also, we believe, been insisted on by Dr. Playfair in a paper on this subject published within the last few years.

The observations made in this case led Feltz to perform certain experiments with the object of ascertaining whether similar phenomena would manifest themselves in the lower animals under similar conditions. Two rabbits were plunged into boiling water, one as far as its forelegs, the other only up to its middle; and they were kept immersed for about two minutes. One died ten hours afterwards. The veins of the scalded parts were found to be full and hard, and could with difficulty be emptied by the pressure of the finger. Microscopically the vessels of these parts were observed to contain reddish-black deposits, composed of shrivelled blood-corpuscles, which resembled those of dried blood; the fibrin was coagulated in the form of more or less granular irregular patches; it did not display its ordinary fibrillated appearance. The lungs were of a pink colour, but presented purpuric spots, which extended some distance into their tissue. These further contained deposits similar to those found in the scalded parts—shrivelled blood-disks, masses of opaque, granular, more or less pulverulent fibrin. The other rabbit lived two days; no very definite changes seem to have been discovered in its lungs.

Feltz goes on to say that the occurrence of pulmonary lesions as a result of burns had previously been observed by Dr. Wilks. Indeed, in his important paper on pyæmia, in the seventh volume of the 3rd series of the 'Guy's Hospital Reports,' Dr. Wilks gives four cases, in two of which the kidneys contained masses of "fibrinous material." But on the general question he remarks:

"We can scarcely give an opinion as to the frequency of pyæmia as a result of burns and scalds, as a very large number of careful inspections would be required. Our experience, however, so far as it goes, is of the same kind as that generally held, that pyæmia is far less common than might be expected from the extensive nature of the wounds often witnessed after these injuries."

In leaving the subject of the production of dyspnœa or sudden death by capillary embolisms of the pulmonary artery, it may be as well to pass on to the cognate subject of the production of sudden death by capillary embolism of the cerebral arteries. For it is to be noted that (with the exception, perhaps, of the cardiac arteries) these are the only branches of the aorta in which the circulation is so necessary to life that its stoppage is likely to give rise to an immediately fatal result.

And here, again, as it appears to us, Feltz altogether fails to

prove that in the human subject death can result immediately from capillary embolism.

The only case in point contained in the work under review is that of an old woman, æt. 72, who was admitted into the medical clinique of Strasbourg, under the care of Prof. Coze, for rheumatic disease of the heart, with a direct aortic murmur. She was found dead in her bed and cold at 5 a.m. one morning, and as she had been heard to cry out about midnight, it was supposed that she must have died suddenly.

On post-mortem examination, the cerebral arteries were found much diseased. The basilar artery contained a dark red coagulum, in the interior of which was a whitish fragment, very similar to pieces which could be picked off the aortic valves with the finger-nail. This fragment was composed of a cretified atheromatous substance; calcareous grains could be plainly recognised in it. When the different cerebral arteries were traced onwards, the minute branches towards the capillaries were found to contain a very fine fatty dust, and certain grains apparently composed of calcareous salts.

As we have already stated, this case is put forward as one of sudden death from *capillary embolism* of the brain. We hardly think that any clinical observer, who bears in mind the frequency of sudden death in cases of disease of the aortic valves, will attach much value to it, standing as an isolated instance. The most that can be said is that it opens a promising path for future investigation.

The next case given by Feltz is quoted from the 'Gazette des Hôpitaux.' It is that of a child affected with chorea, who was attacked with left hemiplegia as a result of embolism of the right Sylvian artery, by a plug of fibrin detached from the mitral valve, as was shown at the post-mortem examination. At the posterior part of the right hemisphere, in the path of a small artery, there was also found a small, elongated, white spot, surrounded by a slight zone of capillary congestion, and due to a thrombus, which could easily be picked out. Here and there, in the intervals between the convolutions, the pia mater presented an intense congestion of its vessels, so that it was thickened and of a deep violet-red colour. In the centre of these islands of congestion vessels were found to be obliterated by thrombi. This state existed in both hemispheres.

We have quoted the appearances found in this case at length, not because they appear to have much bearing on the points discussed by Feltz, but because of their great interest in reference to the view maintained by Dr. Broadbent and Dr. Tuckwell, that chorea itself may result from an embolism of certain of the finer cerebral arteries. The view in question, however, is not mentioned in Feltz's work, and therefore we abstain from any further reference to it in this place.

We have shown how very narrow is the *clinical* basis which

alone at present supports the opinion that capillary embolism of the cerebral vessels can cause sudden death.

It remains for us to consider the *experimental* evidence which is brought forward by Feltz in reference to this point.

This experimental evidence consists in the results of the injection of pulverulent substances into the arterial system by means of a canula introduced into one carotid artery, and long enough to reach down into the left ventricle. The effect of this operation was in some instances very striking. Let us take, for example, the fifth experiment of the series in question. A dog of moderate size has injected into his left heart 8 c. c. (about two drachms) of pus. The operation is half completed when the animal presents transitory convulsions, after which the emptying of the syringe is completed. The animal is then let loose, and is unable to stand; it has repeated attacks of clonic convulsions, followed by a tetanic condition, which soon terminates in death. The head is at once examined. The brain is pale; its arterioles and capillaries are full of pus. In the venous blood no trace of the injected matter can be found. The whole of the pus has not entered the brain, for the blood of almost all the organs contains it in greater or less quantity.

This experiment is one of five, in all of which the operation was followed equally rapidly by a fatal result. In the other experiments, however, rabbits were employed; the material injected was in two instances pus, in one case a cancerous liquid, in another distilled water containing powdered fibrin in suspension.

These experiments are of some interest, as showing clearly how untenable was the opinion at one time held (with reference to pyæmia) that pus when injected into the blood at once causes coagulation, and thus that it cannot in fact penetrate into the circulating fluid.

It is, however, to be noted that the five experiments in which a rapid death was produced by the injection of pus or fibrin into the left heart were the only ones in which this result followed, out of fifty in which the same operation was performed. This not only shows how difficult it is to produce such a result, but also how very little practical value can be attached to the fact that it should sometimes have been arrived at; and it is particularly to be noted that in some other experiments, when the material was merely thrown into the carotid artery in the direction of the circulation, no immediately fatal consequences were observed. The conditions required for the production of sudden death by the entrance of finely divided substances into the cerebral circulation are, in fact, such as could scarcely be met with in the sphere of clinical observation.

We have now to discuss a far more complicated and difficult matter—the production of pathological changes in the tissues served by arteries and capillaries which become the seat of emboli. Under this head French and German writers include, not merely the “fibrinous blocks” or “wedges” so common in the spleen and kidneys, but also the various forms of “pyæmic deposit” and “abscess.”

As might be expected, a change so striking as the presence of a “fibrinous wedge” in the spleen did not escape the notice of the older pathologists. Cruveilhier gave a capital description of this affection. He called it “infiltrated splenic tuberculization.” But it is to be observed that no tubercle in any other organ existed in the case to which Cruveilhier especially refers—that of a girl who had disease of the heart. Rayer termed the affection a “néphrite partielle rhumatismale.” Barth attributed it to interstitial hæmorrhage.

By modern pathologists, however, the occurrence of these wedge-shaped patches in the spleen and kidneys has long been associated with embolism. And in French and German works of the present day they are uniformly termed *infarctus*.

This name *infarctus*—derived from the Latin verb *infarcio*, to stuff or fill—is no new-fangled one. It was formerly much used by physicians of the humoral school, to signify visceral obstructions in general, and vascular obstructions in particular. Laennec called the lesion now known as pulmonary apoplexy “*infarctus hémoptoïque*,” and he also spoke of “*infarctus tuberculeux*” and “*infarctus péripneumonique*.”

The authors whose works we are studying speak continually of *infarctus*. So universal is the use of this name in France that Feltz deems it unnecessary to give a definition. “Every one knows that it designates the parenchymatous lesion secondary to capillary embolism.” Lefevre defines a “visceral *infarctus*” as follows:—It is “the anatomical alteration of a greater or less portion of the parenchyma of a viscus, supervening, through defect of nutrition, on the obturation of the arterial branches supplying it.”

Now, it is important that the reader should understand that the use of the word “*infarctus*” is by no means limited to the wedge-shaped masses in the spleen and other organs hitherto referred to. On the contrary, there are included under this name two lesions generally regarded in this country as distinct, the so-called “pyæmic deposits” in the lungs and other organs, and the masses of “pulmonary apoplexy.” All these are believed by foreign pathologists to result from the obstruction of the minute vessels of the part. But the case of “pulmonary apoplexy” is supposed to differ from the others in the circum-

stance that the artery is plugged, not by material brought from a distance, but by coagulation *in situ*; it is due, not to "embolism," but to "thrombosis."

To many of our English readers the association under a common name of these changes, so varied both in their physical characters and in their clinical significance, will probably appear forced and unnatural, and may seem to require a justification, which is not needed in those continental countries where the habitual use of the word *infarctus* has accustomed men's minds to the idea that all the affections included under it have a common nature.

It may therefore be well that we should quote a few passages from some modern English writer on the subject, and fortunately we have ready for our purpose a very able paper, the article on "Pyæmia," in the first volume of 'Dr. Reynolds's System of Medicine,' by Dr. Bristowe:

. . . . "Throughout the whole series of allied but not identical processes (says this writer), we find that obstruction of the supplying artery causes in the part supplied precisely those lesions which occur as the specific local manifestations of pyæmia. . . .

"The pulmonary apoplexy attendant on heart disease is, we believe, generally, if not always, due immediately to thrombosis of branches of the pulmonary artery belonging to the apoplectic region, followed by congestion and rupture of the capillary region of the part. Now, the clots of ordinary pulmonary apoplexy are almost identical with the pulmonary clots of certain cases of pyæmia. Like them they become decolourised at the surface; like them they may become more or less perfectly surrounded by a rim of softening or suppuration, or may present similar changes in their interior; and like them, when they abut on the surface of the lung, they lead to the deposition of a layer of fibrin on the overlying pleural lamina. But, as a rule, they are less numerous and much larger than pyæmic clots, and their tendency to soften, to suppurate, to slough, is far less. In the spleen the wedges or blocks of effused blood, or fibrinous exudation, which so commonly result from cardiac emboli, are, in their general aspect, almost exactly like those connected with pyæmia; but here again, the tendency to rapid suppuration or decomposition distinguishes for the most part the one form of deposit from the other. In the case of the kidneys the differences are less pronounced; in both affections minute abscesses are of common and early occurrence; in both, hæmorrhages and exudations of fibrin alone are occasionally met with. It would seem, then, that the chief distinction between pyæmic deposits and those resulting from simple emboli resides in the fact of the greater tendency of the former to undergo changes of degeneration and destruction. But this, after all, is chiefly a difference of degree, and the difference in this respect between them is no greater than the difference which may often be observed between actual cases of pyæmia. Thus,

in one case of pyæmia hæmorrhagic affections will be alone discovered in the internal organs, in another case patches of inflammation only, in a third abscesses, in a fourth gangrenous excavations; though more commonly doubtless these various conditions are to a greater or less extent commingled."

We shall subsequently advert to the cause of the differences pointed out by Dr. Bristowe in the above-quoted paragraphs. But before doing so we desire to draw the reader's attention to some very interesting observations that have recently been made as to the early characters of infarctus in general and their relation to the arterial obstruction which causes them.

Nothing, in fact, is of more importance in regard to the theory of this subject than the determination of the early characters of infarctus, implying, as it does, the question whether plugging of an artery is or is not followed by that anæmia of the supplied tissues which at first sight would be expected to follow such an occurrence.

But, as Lefeuve remarks, the statements with regard to this point made by writers on the subject have varied greatly. Some, among whom is Cohn, have believed that a commencing infarctus has different characters in different organs; that in the spleen it is red from the first, in the kidney pale. According to others, the early appearance is the same in all organs; but these again differ among themselves, for whereas Rokitansky describes an infarctus as at first red, afterwards becoming pale by a decolorisation of its red globules, Beckmann speaks of it as being originally colourless, and either remaining so to the end, or becoming accidentally red by secondary collateral hyperæmia, or extravasation of blood.

Now, in the course of some experiments on the lower animals made by Prévost and Cotard, in reference to cerebral softening, those investigators conceived the happy idea of laying open the abdomen immediately after injecting foreign bodies into the arterial system, and so of tracing from their first origin the development of infarctus.¹ The effect of this procedure was that in about a minute a raised patch made its appearance on the surface of the spleen, and rapidly increased in size, till it became as large as a two-franc piece. Its edges projected like those of an erysipelas. The patch was of the same tint as the rest of the organ. Prévost and Cotard were able to determine that the corresponding branch of artery was obstructed.

It does not appear, however, that these investigators took the trouble to repeat the observation. Its full importance seems to have been first appreciated by Lefeuve, when he after-

¹ 'Gazette Médicale,' 1866, p. 202.

wards took up the subject of infarctus, and he succeeded in obtaining the assistance of Prévost, who made with him the following experiments :

I. Some seeds of tobacco were injected upwards into the right crural artery of a dog. The abdomen was opened immediately afterwards. The external surface of the spleen was seen to present seven violet elevations (*bosselures*), which quickly increased in size. At the end of two minutes some of them were raised half a centimètre above the surface of the organ. At the autopsy seven small arteries, corresponding to the violet spots, were found to be plugged with tobacco seeds.

II. The abdominal cavity of a dog was opened and the spleen exposed, which thereupon shrank to half its volume. Tobacco seeds were next injected down the carotid artery. At the end of one to two minutes the spleen was seen to present three violet turgescient patches, and in the artery going to one of these patches tobacco seeds could be felt through the peritoneum. At the end of half an hour the dog was killed, and its dissection was at once commenced. When the vessels of the spleen were cut across, in removing the organ, the tumefied violet patches at once subsided and disappeared, leaving no trace of their presence. It was also in this experiment that the production of infarctus in the living kidney was studied for the first time.¹ The capsule of this organ is too opaque in the dog to allow of their being seen through it; but Lefeuve conceived the idea of removing this, and succeeded in stripping it off without giving rise to much hæmorrhage. The organ, as a whole, was congested, and of a dark brown-red colour. Even when first exposed, it presented certain anæmic spots; these afterwards became paler and paler, and at the end of fifteen minutes were of a uniform gray-yellow colour, neither raised nor depressed above the surface of the organ.

III. In this experiment attention was especially directed to the kidney. Tobacco seeds were injected, and the spleen was watched until three brown turgescient spots appeared on its surface. The left kidney was then decorticated. It was already marbled with spots of a very deep red-brown colour, and with pale faintly rose-coloured spots. These were limited by sinuous borders, intersecting the organ as in a piece of *marqueterie*. The dark patches were then marked with a ring of nitrate of silver. One of the most obvious among them was subsequently found to correspond with two interlobular arterioles obstructed

¹ Prévost and Cotard say that in their experiment (above quoted) ecchymotic patches appeared here and there in the kidneys; but it was difficult to determine whether they occupied the parts supplied by the obstructed arteries, or those lying between them.

by seeds. Other brown patches likewise seemed to have grains in the arteries supplying them, but with less absolute precision of correspondence. The arteries supplying the pale patches were permeable.

A comparison of the results of Experiments II and III would seem to show that they left some uncertainty as to the early characters of infarctus in the kidneys. But Lefevre sums up his observations as proving that both in the spleen and kidneys infarctus are at first of a dark colour. In the latter organ they are raised above the surface; in the former, they are flat.

But whatever discrepancy there may be in the account given by Lefevre would seem to have been removed by the observations of Feltz, who has repeated his experiments, and with the result of entirely confirming his statements. It may be added that Feltz gives details as to the size of the infarctus observed by him, a point omitted by the other experimenter. In one instance the spleen had dark red patches, varying in size from that of a small pea to a pin's head; the kidney, one spot, of the size of a large pin's head. In another experiment the spleen showed three very turgescient infarctus from a half to one centimètre in diameter.

Feltz appears to have been the first to trace the formation of infarctus in the living lungs. The thoracic cavity of a dog was opened, and artificial respiration was set up. Tobacco-seeds were then injected into the right heart. In three or four minutes blood-red spots appeared on the surface of the lungs, contrasting sharply with the pale rose colour of these organs.

Another part in which the development of infarctus has on one occasion been watched is the small intestine; and here, in the first instance, a pallor of the affected parts did really manifest itself. The experiment was the second of those made by Lefevre. The account given by him is that the small intestine for ten to fifteen centimètres became pale. The loop which underwent this change belonged to a group of divisions of the mesenteric artery filled with tobacco-seeds. It was watched attentively for fifteen minutes. The next thing observed was that the veins corresponding to the obstructed arteries became distended with black blood. Then the collateral arteries (which were not obstructed) became filled with an increased quantity of blood, which distended their finest capillaries, both those in the mesentery and those in the gut itself. This hyperæmia gradually extended by means of the anastomoses of the vessels; but its advance was slow, and it could not reach the centre of the anæmic part. At the end of twenty minutes or half an hour the loop began to assume the livid red colour observed in

strangulated hernia. It then seemed that blood was extravasated.

It being, then, determined that the immediate effect of embolism of a minute artery in the spleen or kidney is to cause intense redness and engorgement of the tissue supplied by the vessel, the next question is, How is this effect produced? Does the blood come from collateral arteries, or does it flow back from the veins of the part itself?

Now, there is no doubt that the engorgement with blood which constitutes a commencing infarctus is due to the second of the causes suggested in the previous paragraph. According to Lefevre, it has been shown by Moreau that ligature of the splenic artery causes the whole of the organ to become turgescient, and in this case the possibility of the hyperæmia coming from the collateral vessels is, of course, excluded. Moreover, it is well known that the splenic tissue is divided into a number of distinct vascular departments, each having an independent arterial supply. As far back as 1802 Assolant showed that it was possible to fill these portions of the organ separately with injections of different colours.

It is supposed by Lefevre that the reflux of blood which thus is necessary to the formation of an infarctus is caused by a paralysis of the muscular walls of the vessels, in consequence of their failing to receive the necessary stimulus from the flow of blood through them. At the same time he believes that the muscular fibres in the trabeculæ of the spleen are paralysed, and in this way he accounts for the fact that the infarctus is raised above the level of the rest of the organ. In the kidney, which is not a contractile organ, the affected parts are simply reddened, and not thus elevated. That the engorgement of an embolised part is not derived from the collateral arterial supply is further proved by the fact that in organs (such as the liver) in which the collateral anastomoses are abundant true infarctus are never met with.

It is, however, still a matter of dispute whether this hyperæmia is the sole morbid change in a commencing infarctus, or whether hæmorrhage is invariably superadded. The latter opinion is maintained by Feltz, who asserts that on incising the infarctus produced in his experiments he found them to be plainly hæmorrhagic. In some instances the parts were frozen before being cut into; and not only was blood evidently extravasated into the tissue, but in one instance, in which a carbonaceous powder had been the material injected, deposits of this powder were found in the tissue, "which could be scraped with the scalpel, and had evidently escaped from the walls of the blood-vessels." On the other hand, Lefevre asserts that the

occurrence of hæmorrhage is not an essential, but only an accidental, element in the formation of an infarctus. When it occurs it is due, he says, to friability of the capillaries, resulting from commencing necrobiotic ulceration.

With regard to the subsequent changes that take place in infarctus, there appears to be little difference of opinion. They gradually become decolorised, and acquire a yellow colour from fatty degeneration of the tissues. "In an old infarctus," says Lefevre, "it is difficult to recognise the constituent elements of the organ. These have become granular, opaque, and masked, if not replaced, by innumerable fatty globules."

Still later, as is well known, these elements are absorbed, and in the end nothing is left but a deep fissure or puckering, extending from the surface of the affected part more or less deeply into its interior. The process is, however, occasionally varied by the occurrence of calcification, as has been described by Prévost and Cotard.¹

It was maintained formerly that infarctus of the spleen and kidneys differ from the allied affections ("pyæmic deposits" and "apoplexies") of the lung in the absence of any tendency to the occurrence of suppuration or gangrene; and this was at one time ascribed to the fact that air has no access to the first-named organs. But it is now known that this difference is not absolute. Lefevre mentions a case in which an infarctus in the spleen was sloughing. The organ presented four abscesses the size of walnuts, containing black masses and a puriform detritus; it also had two ordinary yellow infarctus. We have recently seen a case in which the centre of an infarctus in the spleen contained a slough, already detached, larger than a marble. In the year 1868 Dr. Moxon exhibited to the Pathological Society a case of ulcerative endocarditis, in which abscesses, both in the brain and in the spleen, were apparently traceable to infarctus.²

Still, there can be no doubt that in the main a broad distinc-

¹ 'Gazette Médicale,' 1866, p. 103.

² Feltz gives, at pp. 119 *et seq.*, a case which at first sight might appear to be somewhat similar. A man who was diagnosed to be suffering from acute rheumatism, complicated with ulcerative endocarditis, died with abscesses round the joints and beneath the skin. The aortic and mitral valves were found covered with easily separable granulations, beneath which were very superficial ulcerations. These were supposed by Feltz to have been the cause of the other morbid changes found in the body; but a perusal of the clinical history convinces us that the case was much more probably one of pyæmia in the first instance, and that the slight affection of the cardiac valves was the result of this pyæmia, and not its cause. The fact that pyæmia, even when set up by surgical injuries, such as fractures of the bones of the extremities, sometimes causes an inflammation of the valves of the heart with deposition of fibrinous granules, was pointed out by Dr. Hilton Fagge in a communication made to the Pathological Society some years ago; and he then pointed out that a knowledge of this fact makes some care needful for the right interpretation of cases such as those now under consideration.

tion exists between the two principal forms of infarctus, in the comparatively slight liability to suppuration and gangrene, presented by those which in this country have generally been termed "fibrinous blocks" or "wedges," and have been regarded as the effects of embolism. And we may now consider to what this distinction is essentially due.

In the liver, as is well known, infarctus are rarely, perhaps never, met with. Yet floating coagula must frequently become impacted in its smaller vessels: in the twigs of the hepatic artery, from ulcerative endocarditis; in the ramifications of the portal vein, where disease exists in the organs supplying that vein with blood. The non-occurrence of infarctus in the liver may, indeed, be attributed to the peculiarities in its circulation, and to the free anastomosis between its vessels. But then comes the question—How is it that this very organ is particularly liable to be the seat of pyæmic abscesses, if these abscesses are not essentially different from infarctus?

It can hardly be doubted that the answer to this question must be sought in the existence of some peculiarity in the matters by which the vessel is obstructed, or in the circulating fluid in which these are immersed, enabling them to produce effects surpassing those which would result from the mere mechanical closure of the vessel itself.

In almost his earliest paper on plugging of the pulmonary artery, Virchow recorded observations which have a direct bearing on this subject. He had performed on dogs a number of experiments, in which he injected substances into the jugular veins. And when he employed animal substances (portions of fibrin, bits of muscle, &c.), not only did these substances occlude branches of the pulmonary artery, but they gave rise to severe pneumonia, which rapidly passed into suppuration or even into sloughing of the affected parts. As these changes advanced pleurisy was set up, leading to large sero-sanguineous effusions, with a great disposition to the occurrence of putrid changes, and also with a tendency to perforation of the pleura, leading to pneumothorax. In one case the whole of these changes manifested themselves within five days. On the other hand, when portions of caoutchouc were used for injection no such results followed. The dogs remained in perfect health. It was necessary to kill them in order to obtain evidence of the fact that the injected bodies had really entered the pulmonary vessels. It is, therefore, evident that for the production of the more severe inflammatory changes in the parts plugged something more is required than the mere mechanical presence of the plug itself, a something which is afforded by portions of animal tissues, and not by caoutchouc.

In a subsequent article, entitled "Embolie und Infektion," Virchow starts from a different point in discussing this question. In the blood of leukhæmia, he says, cells precisely like pus-cells exist in large numbers. This disease, however, is attended neither with the general symptoms nor with the local lesions belonging to purulent infection. Hence it is a fair conclusion that when real pus-cells enter the blood, the effects which they produce cannot be of a *mechanical* origin, but must result from some *chemical* action. The phenomena of so-called pyæmia may thus be divided into two great groups, of which one must receive a mechanical, the other rather a chemical, explanation; in other words, there are *phenomena of embolism* and *phenomena of infection*.

By Feltz the argument from the facts observed in leukhæmia is carried a stage further than by Virchow, for he endeavours to show that masses of the white cells may actually accumulate in the minute vessels and cause fatal obstruction without being able to give rise to infarctus. The case which he adduces is that of a man who died of sudden asphyxia in the course of this disease. The upper lobes of the lungs were slightly œdematous. When the organs had been cut open, and pressure was applied, there were squeezed out of the blood-vessels a number of small white coagula, resembling cooked vermicelli. Some of these, when shaken out in water, were found to be ramified and to preserve the form of the vessels from which they came. Microscopically they consisted almost entirely of leucocytes. The blood-vessels of the brain, liver, and spleen, contained similar masses. Feltz thinks that, as the patient had by no means arrived at the extreme limit of life for cases of leukhæmia, his sudden death must be attributed either to the capillary embolism of the lung or to cerebral embolism. Yet there was no trace of infarctus. (It seems to us, however, that this case is open to the same kind of criticism which has been applied to other cases of Feltz's which we have already quoted.)

With regard to the relation of infection to embolism, Feltz's views appear to us not to be essentially different from those of Virchow. But it would seem that he regards them as original, and therefore it will be best to let him speak for himself.

"Infarctus of the lungs are sometimes benign, and then undergo absorption, with or without suppuration of the adjacent parts of the lung; sometimes malignant, when they set up extensive suppurations and gangrenes. Modern works accordingly divide them into simple and specific.

"Starting from the tendency of infarctus to one or the other of these terminations, authors have attached their benignity or malignity to the cause which determines them, and have not hesitated to divide emboli likewise into simple and specific.

"For ourselves we cannot admit such a division without reserve. We have injected so many kinds of pulverulent substances and organic bodies of different nature without ever obtaining any result except infarctus which underwent softening and suppuration, that it is impossible for us to allow that embolism produces any but mechanical effects. We do not wish to deny specificity, for we know from our own researches, detailed in our memoirs on infectious diseases (Coze and Feltz) that many substances may infect the economy; but we know also that to produce this result there must be a septic liquid. We do not think, judging from our experiments, that solid elements can carry infection by themselves; we believe that this is always propagated by fluids.

"Injections of septic fluids holding solid elements in suspension produce the two effects, embolic and infectious."

As we have already stated, these views of Feltz and those of Virchow appear to be essentially identical. They have, however, been combated by Panum, of Kiel, and it seems necessary that we should give the outlines of the arguments used by this writer.

On the one hand, Panum recalls the fact that Virchow himself had found great differences in the effects produced by the injection of vegetable substances, according to the character of their surface. As we have already stated, caoutchouc had no irritant action when impacted in the pulmonary artery, but when elder pith was employed it set up the same changes in the lung tissues which are caused by portions of fibrin, muscle, &c.

On the other hand, Panum himself has injected bodies saturated with putrid fluids, but has found that (contrary to Virchow's and Feltz's theory) these simply became encapsuled, and exerted no irritant action.

It was only when bodies themselves capable of decomposition were employed for injection that distinct irritant effects followed. Panum concludes, therefore, that the irritation is set up by the products of such decomposition of the bodies themselves, and not by any putridity of the fluids.

A still later writer, O. Weber, in an article on the subject in Billroth and Pitha's '*Handbuch der Chirurgie*,' declines, however, to accept Panum's conclusions. The phenomena of irritation which follow embolism in certain cases are referred by him rather to the local disposition of the tissues, and to the greater or less completeness with which the circulation is restored through collateral vessels.

In discussing how infection arises in association with embolism, it is further necessary to take into account the suggestion of Wagner and Busch, that the diffusion of fat-globules through the blood may be concerned in their production.

With the object of testing this view, Feltz has performed

several experiments. In the first place, he injected 3 c.c. of an emulsion of olive oil, coloured with fuchsin, into the jugular vein of a rabbit. The animal instantly died asphyxiated. The fatty globules were readily recognised in the small pulmonary vessels, many of which were ruptured, allowing blood to escape beneath the pleura, so as to form dark red spots. The experiment was repeated on another rabbit, and with the same result.

Feltz then proceeded to produce a severe compound comminuted fracture of the leg of a rabbit, but left the periosteum, so as to form a cavity, into which he poured a small quantity of oil. He also broke up the medulla in both the fragments. The animal died thirty hours later, and an autopsy was made without delay. The crural vein was found to contain masses of fatty granulations, precisely similar to those which existed in the medulla of the broken bone. The lungs presented a considerable number of bright red ecchymoses, one fourth to one third of a centimètre in diameter. Certain of the capillaries were gorged with minute granulations, some free, some collected into masses.

A procedure more or less similar to that which has just been detailed was adopted in fourteen experiments, but in only two of them were any effects produced of a confirmatory kind. They do not, therefore, strengthen in a very decided manner the position taken by Wagner and Busch.

In a later portion of his work Feltz carries these experiments somewhat further, with the object of explaining those cases in which a pyæmic condition, starting from the systemic venous system, produces its secondary deposits, not in the lungs, but within organs supplied by the systemic arteries. Such cases it is generally impossible to attribute to ordinary embolism, for the capillaries of the lungs must have been traversed by the materials supposed to be taken up into the blood.

Sometimes, indeed, this difficulty may be removed by assuming the occurrence of what has been termed by Feltz and other writers "*secondary embolism*." Of this Feltz finds an example in the following case:—A man dies of pyæmia after amputation of the leg. The lungs contain a number of infarctus, evidently of some standing, for certain of them are of a gray-yellow colour. There is also a partial destruction of the arytaenoid cartilage, and the laryngeal branch of the inferior thyroid artery is found to be obstructed by a clot. This clot is not homogeneous, but is made up of a recent red mass, containing paler friable masses; and these last are precisely similar to thrombi which lie in some of the pulmonary veins round the infarctus, and which, therefore, are supposed to have been the source of this *secondary* affection.

In another case infarctus and abscesses of the liver, arising from plugging of the mesenteric veins in typhoid fever, appeared to have led to thrombosis of certain radicles of the hepatic vein, and this, again, to secondary embolism and early infarctus of the lungs. The same patient had abscesses of the scalp, which Feltz attributes to a third source, the presence of certain deposits on the mitral valve.

We have quoted these cases in order to show how far such views may be carried by their more ardent supporters. But Feltz himself admits that this theory of secondary embolism should only be made use of when there is a distinct difference in date between the infarctus in the various systems, and that in many cases of pyæmia no such difference can be established.

He therefore devotes a special section of his work to the subject of capillary embolism by masses of finely divided substances capable of traversing the whole circulatory system. The case placed at the head of this section is that of a man who died of malignant jaundice, with delirium, hæmorrhages, &c. The liver was almost healthy. There were numerous hæmorrhages, especially into the mucous and serous membranes and into the lungs. The blood contained crystals, which were at first taken for margarine, but which were afterwards found to be hæmoglobuline. It also contained a large number of small corpuscles soluble in ether, and plates of cholesterine. Feltz therefore imagined that the hæmorrhages were due to the arrest of these bodies within the capillaries, and not merely to the decomposition of the blood.

He consequently proceeded to make some experiments with the object of determining the formation of molecular fat in the blood, and so, if possible, producing infarctus of the organs. For this purpose he administered phosphorus to animals, thinking that this body would probably produce steatosis of the blood, as well as of the solid tissues.

The result of these experiments was to show that the blood contained crystals and brilliant globules of fat in more or less considerable quantity. The crystals were acicular, some of them isolated, some grouped together, and some even radiating from a centre.

It will be at once understood that the hæmorrhages which are well known to be found in such large numbers in animals poisoned by phosphorus are regarded by Feltz as infarctus, and attributed by him to embolism of the capillaries by the crystals and fatty globules just described. The explanation is, no doubt, ingenious, but we must confess to some hesitation in accepting it.

VIII.—Sanitary Administration.

HAPPILY there are now few reasoning persons who do not readily admit, without requiring formal evidence or logical proof, that there is such a thing as Public Health, distinct from personal or individual health, and that it needs preservation or restoration by public or national agencies, as distinguished from the personal care which individuals may be capable of bestowing on their own health. We now know also that, although the state of each individual contributes its statistical quatum to form the general average, that average, if correctly calculated, determines the condition of health, and therefore the collective strength and vitality of the nation or community. How numerous and complicated are the elements of social existence on which the true health of a people depends, has been lately told us by Dr. Acland,¹ with his accustomed felicity of expression and richness of illustration.

The relations of national health to public morals, to civilization, to laws and institutions, to education in its larger sense, to mental vigour, to race and hereditary tendencies or taints, to food, customs and habits of life, and to international commerce, are as numerous as they are complex; and, in their varied combinations, make any positive conclusions exceedingly difficult.

The subject naturally tempts an acute and comprehensive thinker to enlarge the limits of the field of discussion; and on finding that a really sound state of public health may depend upon so many circumstances and conditions which no improvements in law and administration can directly or presently affect, he may be led by a sort of philosophical indifferentism to undervalue specific measures and normal projects, and to leave these matters very much to whatever may be the ruling power, content that where, as in England, that power is virtually a democracy, it shall be guided by a strong central dictatorship. Here, as elsewhere, extremes meet, and government by the masses naturally merges into imperialism, especially in the higher concerns of local administration.

The principles of sanitary law laid down by Dr. Acland are for the most part indisputable—

“That no individual should, for his own profit, poison his neighbour.

¹ ‘National Health,’ by Henry W. Acland, F.R.S., &c. &c. Oxford and Lond., 1871.

"That the State must, in certain things essential for health, assist the masses in what they cannot assist themselves.

"That the cost of permanent sanitary improvements should be borne, in some reasonable proportion, by posterity.

"That compulsion of the ignorant in sanitary matters, when their ignorance injures society, is justifiable.

"That compulsion will, we hope, be unnecessary when scientific education is adequately extended.

"That good conduct, based either on knowledge or obedience, is as essential to health as is any physical arrangement which is not an actual necessary of life

"That in the present state of the world mischief-bringing ignorance in sanitary questions is especially inexcusable in the law-making classes.

"That local government by the people, well informed by a central authority, is essential to the physical education of the nation."

To the last only of these propositions do we take exception, partly because we do not quite understand it, and partly because we believe education, physical and moral, of the people to be an essential preliminary to any rational system of local self-government. If such an education had been previously bestowed and accepted, there would be the less necessity for information, still less for compulsion, by "a central authority."

The peculiar form which democratic rule takes in this country is determined by the local aggregations of the people.

In the early part of the present century the cities and towns of England contained barely one third of the whole population; but now, probably, the proportion is reversed, and nearly two thirds are collected in urban districts, which dictate the course and method of imperial legislation. And these town masses consist of individuals who are assumed by some to be collectively capable of preserving their own health. The saddest consideration, however, is that, under present conditions, just as each person comes to lose his individuality in the collective, so do his powers of maintaining health, prolonging life, and improving mental and bodily vigour, really diminish.

The individual may, on the whole, manage to avoid—whether by instinct, by reason, or by precept—many causes of disease and mortality, so long as he can act singly. His right of self-preservation is not interfered with by others.

But, no sooner are human beings massed, without culture and control on right principles, than their individual powers of self-preservation become crippled, while most of the collective actions of the mass tend to enfeeble, sicken, and destroy each constituent. In proportion to the progress of this evil does the community feel less and less disposed to interfere with consequences, to which, indeed, it gradually becomes blind. Results are accepted reck-

lessly, sometimes gaily. "Let us eat and drink, for to-morrow we die."

Habits which defile both body and soul, sordid motives of gain, customs which deaden the sense of right and wrong, and which create selfish interests injurious to others,—all conspire to render such a community not only unable to devise and unwilling to enforce any preventive or restorative regulations for themselves, but also averse to any such control by a superior or external authority as might arrest the downward course.

There exists, indeed, an internal corrective power in nations and societies which have not decayed, which contain enough of the higher and better elements to influence and lead public action, and which, therefore, are not governed by the nominees of the lowest and most degraded of the people.

But why do we repeat what may be called truisms? Because there are those who, not recognising the sources, remote indeed, of their own knowledge and civilization, and anxious to acquire influence and popularity, would persuade the ignorant masses that they are capable of evolving from their own obscured faculties those social truths and laws which are necessary to their well-being. This is self-government without education and wholesome tradition to guide it, without the control of the nobler and more cultivated intelligences.

What must be the consequence of leaving the care of the public health to the operation of this principle? Yet there are generous minds whose trust in human nature seems illimitable, and who, from various motives, are disposed to leave the administration of the law, yet not without central direction, to bodies elected by uneducated majorities and in bondage to petty interests.

When, for instance, Dr. Acland says—

"We hear that the middle class of England is inefficient, the guardians of the poor bad, and the working classes ignorant. If so, they are the people, they and their children pay the penalty of disease and vice. Show them, truly and without exaggeration, the source of avoidable disease, and of destructive vice, they will abate it. Bring the knowledge to their doors, they have heart and will; give the power by enactment, and the work is done"—

it seems to us as though his charity had dimmed his recollection of comparatively recent facts.

Have not "the sources of avoidable disease, &c.," been shown to the people and to municipalities, for the last thirty years, by every method of public announcement? Yet have those sources of evil been generally or materially abated?

Has not the knowledge been brought to their doors? Yet have they "the heart and will" to apply it?

Have not ample powers for good been conceded to local bodies of all sorts and shapes? But is "the work done"?

If it had been, what need for another Royal Commission?

II. The evidence taken by this Commission,¹ though unavoidably defective, is so plain and so consentaneous, as to the inefficiency of existing local authorities, that one wonders at some of the conclusions at which the Commissioners have arrived. But of these on another occasion.

Now, among the 101 witnesses examined, there are some representative men of most classes—heads or chief advisers of central departments in London, Edinburgh, and Dublin; inspectors, medical, legal, and engineering; parliamentary counsel and high legal authorities; many chairmen and clerks of town councils, local boards and guardians; land agents and solicitors to sanitary undertakings; noblemen of great experience in Government office and county management; other landowners, magistrates, and M.P.'s; parochial clergy; inspectors of nuisances; millers and ironmasters; members of the Commission itself; a few physicians known as writers on preventive or public medicine; and seven medical officers of health, of whom three only belonged to that part of the United Kingdom to which the inquiry applied, and two poor-law medical officers. Although the oral evidence thus taken was necessarily limited to a small proportion of the towns and districts of England, it tends to prove that, with few exceptions, the local administrative bodies, especially in the smaller towns and rural districts, are utterly incompetent to execute sanitary measures, and generally resolved not to incur any expense about them.

Towns and cities of the first magnitude, whether under their own local acts or under the local government act, make a more respectable appearance in the report; but then it is to be observed that those examined on behalf of Liverpool, Manchester, Birmingham, Newcastle and Bradford, were chiefly legal gentlemen holding office under the town councils, who could hardly do less than make out the best possible case for their corporate clients. But there is another side to the question in each of these towns. The very able report of the 'Lancet' Sanitary Commissioner had not long exposed the true state of affairs in Liverpool—and horrible was the picture²—when Drs. Parkes and Sanderson were appointed by the corporation to inquire into complaints respecting the foul heaps of mineral and ash-pit refuse which form the foundation of recently built dwelling houses; the condition of sewers as to con-

¹ 'Second Report, Royal Sanitary Commission,' vols. i, ii, and iii, part I. 1871.

² 'Lancet,' 1870, vol. ii, pp. 674, 710, 794, 893.

struction, deposits, and pent-up poisonous gases; and the general sanitary state of the inhabitants. The report of these gentlemen, though most cautious and as little inculpatory as possible, leaves no doubt that the allegations of the 'Lancet' commissioner were well founded, and that far more has to be done towards reducing the unhealthiness of Liverpool than its corporation and officers admit.

As to Manchester, some erroneous statements of Sir Joseph Heron have been ably and conclusively replied to by the committee of the Manchester and Salford Sanitary Association. We have no space for more than a reference to the valuable report of that body for 1869, which, with the rest of their series, deserves the careful perusal of every student of hygiene.

The statements of the Town Clerk of Birmingham in praise of its healthy condition and its excellent system of inspection, should be compared with the startling revelations made by Mr. George Godwin, F.R.S., at the meeting of the Social Science Association at Birmingham in 1868, only a few months before this inquiry. Mr. Godwin, a well-known authority on such matters, had the courage to undertake a personal investigation of some neglected quarters of that town¹ accompanied by the chief officer of police.

His testimony to the prevalence of the grossest abuses in these filthy localities is unimpeachable. The inferior quality and injurious effects of the water supply, whether by the Water Company or from wells, were also exposed on the same occasion by Dr. Alfred Hill, Mr. Postgate, and Mr. Scofield.

The valuable information given by Dr. Budd respecting Bristol relates more to principles of sanitary administration and to the special advantages of the disinfecting process adopted in that city under its energetic health officer, than to the efficiency of the governing body.

Other large boroughs of a second class—as Yarmouth, Swansea, Oldham, Gloucester—report, through their local authorities and officers, tolerable activity and efficiency; but all confess to suffering more or less from the confusion and clashing of jurisdictions, and from the maintenance of abuses by interested parties. All these populous towns boast of their extensive works of drainage; yet all ignorantly or perversely discharge their excretal and manufacturing impurities by sewers, either into the sea or into rivers and streams passing the towns.

In Nottingham also, where "very serious evils exist," according to Mr. Huskinson, "the sewage is all poured into the river

¹ 'Transactions Soc. Sci. Assoc.,' 1869, pp. 476-7.

Trent. Proceedings are about to be commenced in chancery against the town. Yet Nottingham can do all that is necessary without having to pump."

In the northern boroughs generally, although provided with sewers, the ash-pit-closet system maintains its supremacy, numerically. This system, in its ordinary unreformed condition, is shown by the Manchester committee, to whose labours we have referred, to be the cause of much of the unhealthiness and mortality in that city. But it is admitted by the same committee, and asserted confidently by the local authorities, to be capable of such improvements by reducing the size and altering the construction of the receptacles, by the constant use of fine ashes for disinfection, and by a very frequent, if not daily, removal of their contents under skilled direction (a dry earth or ash system in fact), as both to protect water courses from defilement and to utilize the manure, while raising the standard of public health.

The unavoidable necessity for the cartage of large quantities of ashes from these great centres of manufacture, and the dearth of water, seem to point out alike the economy and the sanitary advantage of applying those ashes, scientifically, to closet disinfection before removing them to depôts for agricultural use.

Among boroughs of this class, Lincoln is said to boast the unenviable distinction of still authorising cesspools even for new houses. Yet, it must be allowed that the difficulties attending the utilisation of sewage in a flat country, intersected by sluggish streams and stagnant dykes, are all but insuperable. Local apathy and agricultural torpor, backed by engineering hostility, have hitherto deprived the dry-earth system of a fair trial in these districts.

Some of the minor boroughs are shown to be in a desperate condition. Stafford, for instance, according to Dr. Day's description, more vivid than nice (7294), seems to be a model of beastliness. In the towns of Monmouth, Tiverton, Basingstoke, Chichester, and Seaford, there flourished all sorts of abominations: both soil and water are corrupted by soakage, and therefore neither wells nor natural streams afford a wholesome, barely a drinkable, water, while the atmosphere is reeking with impurities. Certain local improvements reported by gentlemen connected with Knaresborough and Llanelly seem to be of a doubtful character.

The history of drainage works in the latter furnishes a curious fact respecting an outbreak of cholera which is said to have occurred in the highest and best-built part of the borough, and was attributed, not without reason, to the escape of sewer gas ascending from the lower district, through an imperfectly

ventilated main sewer. The ventilating shaft was accordingly abolished ! (3260).

Non-corporate towns—as Tonbridge, Dorking, and Ulverstone—have suffered yet more severely from neglect, mismanagement, and conflicting authority. The story of a long and weary struggle against parochial opposition and jobbery is well told in the evidence respecting Tonbridge.

Even pleasant Surbiton, where the outlay has been great, has come to a dead lock, from inability to dispose of its sewage, the protecting authorities of the Thames and the metropolis being too strong for this unfortunate little suburb. It deserves notice that the local Act under which Surbiton was drained incorporated Sec. 24 of the Towns Improvement Clauses Act, which confers on towns the right of drainage into the sea and public rivers. This practice was therefore encouraged by law in the early days of sanitary reform, when, as the local chairman, Mr. Walpole, said, “it was considered right to go into a river with your sewage.” (5222.)

III. The inhabitants of the smaller towns and semi-rural districts of England are mostly described as living in defiance of all sanitary principles.

Stow-on-the-Wold furnishes a specimen of the absurdities of vestry administration, embittered by party spirit. This parish, with an area of only sixteen acres, has been the scene of profitless conflict between would-be authorities, equally incompetent, while nuisances were permitted to abound and flourish.

Here, as in other districts of the Cotteswold, on the Oölitic formation, sewage is disposed of by the people digging “till they get to a ‘swilley,’ or fissure in the rock, into which the sewage can run, and from which it oozes into the wells.”

In Epping, with a high death-rate, plenty of typhoid fever, and a “special drainage district” to boot, a succession of futile attempts at local improvement ended as we stated before (vol. xcii, p. 291) in the performance of the necessary works by the Secretary of State.

The guardians of Bingham Union, Notts, have utterly failed (4905, 4921) to provide sanitary inspection, and to put into force the powers they possess for the removal of most offensive nuisances. The people of East Bridgford, a parish in that union where fever is constantly recurring, drink either the water of the Trent after it has received the sewage of Nottingham, or the water of wells in porous soil, through which soakage takes place from foul accumulations. The story of the deadly fever of Terling is too well known to need recapitulation. The main practical lesson to be learnt from it is the value and efficiency of the police in sanitary inspection.

Biggleswade in Essex is reported by Mr. Weale as a remarkable instance of small-town opposition to necessary measures of reform, even after the facts of the case, the prevalence of abominations and the consequent injury to the inhabitants, had been most clearly laid before them by an intelligent inspector. In fact, they had received the very instruction which the most enthusiastic educationist could have wished, but it gave them not "heart and will" to abate the evils in their midst.

Saxmundham is another case of dead lock. Here the vestry, having unfortunately made a sewer, could not find an outfall, without risk of chancery injunction, although it seems that the Local Government Act Office had supported a project for discharging the sewage into a brook, only at a distance "further on"! (5593). When will sanitary authorities accept the fact that earth, if properly and promptly applied, is a surer and safer disinfectant than water?

From the flat country near Oxford there is some useful evidence by an eminent surveyor and land agent. He shows what may be done in even a small village to secure a good water supply (6044); and how deficient that supply is in most villages, well-water being in many places quite unfit for drinking. He advises an arterial drainage of land, protected as in old times from sewage and from animal or farmyard refuse. He advocates an improved privy system under proper inspection, and frequent removal of contents. He mentions deplorable cases of overcrowding in miserable cottages. There is no check, he says, upon the construction of dwellings unfit for human beings by squatters and small builders.

Perhaps there is no evidence in this report more instructive as to rural districts than that of the Rev. W. Beckett, a clergyman in the Docking Union, Norfolk, thoroughly conversant with all matters of sanitary and medical care. He proves the fallacy of nominal inspection, either by an incompetent and badly paid "Jack of all trades," or by an inefficient and timid medical officer. He shows the unfitness of existing local authorities to regulate the sites and construction of labourers' dwellings, to provide a supply of good water, to control overcrowding, to abate or remove nuisances, and to carry out any proper system of medico-sanitary care. Like most practical cottage-reformers, he prefers a well-regulated privy system to imperfect attempts at sewerage, and would employ the contents of closets directly upon the land (6557). He also adduces instances of fever from the pollution of wells, and condemns casual drainage into open ditches, which in his neighbourhood trail their loathsome length through marshes into the sea. The building of bad cottages *on bad sites* by unscrupulous speculators, he con-

demns as the source of the greatest injury to the public health. His evidence on the administration of medical relief deserves candid consideration by the Association of Poor-Law Medical Officers, and his advocacy of a good dispensary system may promote their useful project of reform (6602—6619).

Rural sanitary management under boards of guardians was not without powerful defenders before the Commission. Mr. Rowland Winn, M.P., chairman of a large board in Lincolnshire, and Lord Penrhyn, chairman of two or three boards in North Wales, believe that all such sanitary regulations as they deem necessary, and these are not many, might be carried into effect under the authority of the guardians; but then the standard of sanitary administration upheld by these magnates is evidently low. Lord Penrhyn would leave the local boards untouched as sanitary authorities over their small districts; Mr. Winn would place them under the board of guardians of an extensive union.

A different view of the condition of villages in Lincolnshire, as well as in Surrey, Suffolk, Nottinghamshire, Leicestershire, and Derbyshire, is presented by other gentlemen, who describe it as disgraceful, and irremediable under existing authorities. We particularly direct attention to the evidence of Mr. Huskinson, an inspector under the Inclosure Commissioners, and of Mr. Snowball, chief agent to the Duke of Northumberland.

In the rural districts generally *there is no application of sanitary law.*

The embarrassments which attend upon the uncontrolled transition of districts from a rural to an urban character are very forcibly described by Lord Egerton of Tatton. He has observed and suffered by this change, as it proceeds with marvellous celerity in Lancashire, and especially in the neighbourhood of its manufacturing capital. He dwells much on the discharge of refuse into natural drains and brooks, which are thus rapidly converted into common sewers, and become public nuisances on the largest scale. He also objects reasonably to small local boards which cannot afford to employ proper local officers. Mr. Dyke, of Merthyr, and Lord Penrhyn also describe the sad results of allowing new groups of population to settle without any provision for safe and wholesome dwelling-places (6329, 7850).

But throughout the report of the Commission we find no more true and touching picture of the evils and miseries attending upon a rapid and unregulated change from sparsity to density of population than was recorded by Dr. Hunter (in the eighth annual report of the Med. Off. of Privy Council), concerning the new townships which had sprung into existence within a comparatively few years, in the country near Crick-

howell. *Mutatis mutandis*, Dr. Hunter's statement applies almost literally to hundreds of newly forming aggregations in the mining and manufacturing districts of Great Britain, where there is seldom a Titus Salt, or an Edward Akroyd, or a Duke of Northumberland,¹ to direct or civilise the communities they create.

The Commission, indeed, fully recognises (p. 25) "the fatal consequences of neglect" during the rapid growth of such a population. But we fear that their recommendations are but imperfectly calculated to prevent mischiefs, the cure of which is both costly and very difficult.

IV. To revert to urban districts. The Commissioners report (p. 15) that

"Towns and populous districts which have adopted the Public Health and Local Government Acts, or have obtained and acted under special legislation, are much better provided with the requisites for public health than those which have not. Mortality is greatly increased by want of sanitary provisions; a marked reduction of death-rate has followed the improvement of drainage and sewerage, and the supply of other obvious sanitary requirements."

The reference to death-rates tempts us into a discussion, which cannot be entirely shelved, though we have barely sufficient space for it.

Among the fallacies which invalidate this criterion of local healthiness, perhaps the most misleading arise from differences in the kind of area to which the calculation is applied in different places.

It is true that an actual and sometimes very marked diminution in the ratio of deaths to a given population has been fairly ascertained, for a sufficiently long period, to warrant a scientific inference. For instance, in Banbury, Croydon, and Merthyr Tydfil, this reduction is fairly attributed by those who have promoted the improvements to extensive works of drainage and sewerage. Merthyr has also a splendid water-supply, and in every way owes much to its excellent health officer, Mr. Dyke, as does Croydon to the efforts and publications of Drs. Carpenter and Westall.

Nevertheless, here and at Banbury, the pollution of streams, after inflicting more or less annoyance and mischief on the neighbourhood, has been prevented by the Court of Chancery, the injunctions of which, or even the threat of them, have proved a means of safety to many large and populous river districts of this country.

¹ We learn from Mr. Snowball that, in recent lettings of Coal Royalties, the Duke insists upon the most approved dwellings and sanitary arrangements being adopted.

When Sir C. Adderley, in quiet irony, remarked by way of question to the chairman of a local board (5224) that "Surbiton would be none the worse, you think, if the people down the river drank their sewage?" he hinted that the reduction of a death-rate by sewerage of a town into a river is a breach of public morality. We could adduce numerous instances of the fact that such sewerage has been followed by an increase of sickness and mortality on the banks of the river below the town. The case of Salisbury and the Southern Avon¹ is specially instructive, because it shows that, in order to settle some disputed questions in preventive medicine, an extensive area, even a whole river basin, may have to be taken as the basis of calculation. On the other hand, Mr. Rawlinson protested that the returns of mortality published by the Registrar General are utterly irreconcilable with the facts of fatal disease within local-board areas. He mentioned Worthing (664) as an instance of a place being credited by the official register with diphtheria and typhoid fever when there had been no cases of the kind in the town. Such cases, it seems, had occurred two or three miles away, although in the registration area. His vigorous exposure of this source of fallacy recalls many equally remarkable instances of the misleading information conveyed by the mortuary register which have been noticed by sanitary writers. The famous controversy, more than ten years ago, about Liverpool, between the local authorities and Dr. Headlam Greenhow, who had very properly availed himself of the published return from the densely-populated registration district, ended simply in a proclamation of the fact that the borough contained, *outside* of the registration district, some far better conditioned places and longer-lived populations, which of course altered the general death-rate for the whole borough, while they concealed the real mortality of its central and worst portions. The returns are now published for both borough and registration district; but Dr. Greenhow's statement has been amply justified by the recent sanitary history of Liverpool.

Just the reverse took place in Clifton seven years ago. There the death-rate of an extensive registration district, which was very high (owing to its containing several public institutions and some badly-conditioned outlying parishes of Bristol), was quoted by the Registrar General against the town and watering-place of Clifton, standing on a small included area with a really low mortality.²

¹ For the facts of this case, see 'Scientific Opinion,' Dec. 29th, 1869, p. 634.

² Several other instances of either kind of error have been recorded from time to time. See Dr. Rumsey's paper on "Public Records of Mortality and Sickness,"

In most of these cases the defect or fallacy of evidence has arisen from the district or subdistrict being larger than the town or part of town in question. But Mr. Rawlinson rather oddly attributed this vagueness of the statistical reports to "the circumstance that registration areas have been taken which have now no reference to the special purposes to which we want to put mortuary returns for comparison" (665). Did he forget for a moment that the registration divisions were formed long before the new districts? The statistical department certainly is not to blame for the muddle. And in the case of all the great towns the Registrar General and his staff are now doing what they can, in the absence of a scientific organization, to remedy a defect which they had not caused. But this amendment of record does not satisfy the local board party, who urge that the area of registration or statistical return shall be from time to time subdivided so as to correspond with the limits of these continually-forming sanitary districts.

Mr. Simon also has plainly stated, in his 11th Report, his desire that "as soon as possible after the formation of any such district it should be created a district . . . for purposes of registration and census."

Yet it would be easy to show that no statistical system could possibly keep pace with a continual and almost capricious formation of small areas of local government. Evidently, Dr. Farr is aghast at a proposal which would double the toil at Somerset House, greatly enhance the cost of the department, and introduce perpetual uncertainty and change into the statistical record. He declares (5098) that the confusion complained of is due to "those Sanitary Acts going apparently in quite a wild way about the country, cutting the country into little sections; taking parts of parishes and parts of districts, and grouping them together in the most arbitrary manner."

We do not wonder that the Registrar General and the Poor Law Board have objected to the extravagance of the demand for exact statistics from newly formed or altered districts. Dr. Farr recommends, rightly as we think, a general revision of registration areas in conformity with established territorial divisions. This change, though considerable, would be on a general principle, and in the main final. But for purposes of periodical comparison it would be desirable that, for the next decennium at least, all returns from the divided or annexed portions of existing districts should be returned separately. In future, it should be the duty of local scientific superintendents

'Transactions of Soc. Sci. Assoc.,' 1859, p. 574. See also two very instructive articles on "English Watering Places," 'Brit. Med. Journ.,' Sept. and Oct., 1870.

to ascertain and express the numerical results of population movements and district changes, as well as of local efforts. But *an annual census* for this purpose is almost indispensable.

There are inquiries in which a small portion of a sub-district, or an enumeration (census) district, or even a single street or court, may require to be observed and argued upon. This might be necessary to determine the effects of house accommodation. Thus, Dr. Druitt proved to the Commission the necessity of analyzing the population of a district before determining its death-rate. He asserted truly enough that the general death-rate of a parish which contains different classes of society differently housed, is almost worthless. His facts and figures respecting the richer and poorer portions of the Hanover and Belgravia districts of London are most significant. Hence the importance of leaving such calculations to a skilled local observer, who will know when to combine and when to divide.

No rigid adherence to a system of territorial divisions devised by a central authority can possibly meet the exigencies of sanitary inquiry. Neither have we any confidence in attempts, under existing arrangements, to ascertain the exact prevalence, causation, and fatality of particular diseases, in portions of administrative areas, or in particular occupations. At present there is no competent machinery for the purpose; nor can the information be fairly demanded until a medico-sanitary staff is properly organized for the whole country.

While on mortuary registration, we must notice a very important suggestion in the evidence (4278), that in each registry of death the *birth-place* of the deceased should be added, in order to show more correctly the specific mortality of districts and to trace the influence of race, parentage, and early life.

An attempt to invalidate this suggestion was made during Dr. Farr's examination; but its practicability and its actual adoption were curiously enough proved by Dr. Farr's subsequent production (5211) of mortuary registration in use in Victoria (Australia). There, not only is the place of birth (often in England) registered, but also the parentage and the period of residence in the colony before death.

It is to be hoped that the addition of the "birth-place" to the mortuary record or certificate may be included in the measure of reform which we are promised next year.

V. We next propose to treat of those portions of the evidence which relate to the density of town populations and its consequences, and to call attention to some proposals for the mitigation of that original cause of all these evils.

We doubt whether even members of the medical profession,

though so much better qualified than others to form an opinion on the subject, are adequately impressed with the fatal effects of this over-density upon the health, effective strength, and permanent vigour of a race. Yet no principle of sanitary science has been more firmly established by statistical research than that, *cæteris paribus*, sickness and mortality increase with density of population. It might therefore be fairly expected that any reformed code of sanitary law would boldly recognize that principle by effective provisions against excessive and unsafe aggregation of dwellings. On the vital question of distributing crowded populations over larger areas, the Report of the Commission is absolutely silent. The provision or regulation of *sites for dwellings* is not included among the ordinary requisites of "civilized social life" recited in that report (p. 20). This is a surprising omission, at a time when the fearful overcrowding of the lowest classes—not merely in apartments and houses, but especially on acres—is known to be advancing in our large towns, and causing deterioration of health and morals, loss of life and degeneracy of race; also promoting pauperism, drunkenness and crime, and even sapping the very foundations of social order. Hear the evidence of high authorities on this point.

Professor Gairdner, health officer of Glasgow, states that overcrowding *from want of space for buildings* is worse than ever in that city.

"It creates," he says, "special diseases, and it demoralizes the population; and in the course of generations it completely overthrows the physique of the population. It is impossible to walk through the central streets of Glasgow without observing that you are in contact with a population awfully degraded, both in its moral and its physical attributes; a population whose mere external characteristics impress you at once with the idea of a depth of degradation which you can hardly witness in many other places."

He shows that although the authorities of Glasgow have large powers of reconstruction, as well as of purchase and demolition, they have only resorted to the latter; and that while "a great deal of destruction has been going on in Glasgow" from railway operations and other causes, "there has been no reconstruction of houses suitable for those (residents) who have been displaced."

He believes that one third of Glasgow needs reconstruction. His description of a typical house (8252), in which sixty or seventy families live in as many rooms, is appalling. He thus ended a striking description of the demoralization of the young in these dwellings and the neighbouring streets:

"If I were to pitch upon one thing that is the cause of the epidemic disease and physical and moral degradation in that popula-

tion, I should say it was the system of house construction such as we have had for the last three or four generations in Glasgow."

Equally important is the evidence of Dr. Druitt, President of the Association of the Medical Officers of Health in London. He states as the result of overcrowding (8997)—

"A general lowering of health, a degradation of the whole system and character." . . . "Where many human beings are put together *on an inadequate space of land*, they can never get out of the odour of their own breath and of their own excretions" (9001). "There is a general deterioration of the stock, if I may so call it, of the human plant" (9009). "If I go into an overcrowded part, I meet children whose bones are bent and foreheads prominent, and who show all the signs of what is called 'rickets'" (9011). "There is a peculiar look about garret-bred children, stived up in close houses in large towns" (9012). "They may have mental precocity and sexual precocity; I have seen instances of that combined with the worst possible physical development . . . a kind of animal degradation" (9151). "I have seen a healthy father and mother whom you would take as types, living in a confined room, where they had to perform all the offices of nature, living and sleeping and so on, together, and I have seen them produce children which were born apparently healthy, but which gradually withered and became rickety and scrofulous; and therefore, without underrating the influence of vice in the parents, I believe that overcrowding by itself will stunt the human race."

Now, how is all this to end? Is Great Britain, because her town populations deteriorate as they increase, destined to prove her inability to defend herself against powerful and aggressive military neighbours? For where are to be found her future defenders? Are the middle and upper classes to fight and fall in defence of a proletariat five or ten times their number, but unable to help themselves, and ready for any despotism which will supply them with drink, if not food and shelter?

Or, again, are the unhealthy masses to be swept away by hundreds of thousands in a coming pestilence—a just punishment for national disobedience to the fundamental laws of social life?

It is folly to talk conservatively about confiscation and the rights of owners, by way of objecting to compulsory powers for the purchase of land as sites for the safe and decent housing of the poorer classes, when every railway company has obtained a like power of purchase. And what would become of the "rights of owners" were these demoralized and needy millions to feel their numerical power, seize on the habitations of their "respectable" neighbours, or plunder, ravage, and burn after the manner of Parisian Communists?

To return to our evidence. In Liverpool,—where the population is so dense that in one district less than five square yards of land are allowed to each inhabitant (that is, nearly 1000 persons to the acre), and where accordingly the masses are more drunken, sickly, debased and wretched than in any town of the United Kingdom, perhaps excepting Glasgow,—only one fifth of the whole area of the borough is left unbuilt upon; and supposing that it were possible or justifiable to utilize the whole of the unoccupied space for dwellings, it would not effectually lessen the fatal congestion of the masses. Even were the Artisans' and Labourers' Dwellings Act (Mr. Torrens') to be thoroughly carried into effect, it could not cure the evil of excessive population-density, for it confers no powers of extending the inhabited area.

Mr. Rayner is quite right in thinking that density of population is "the paramount cause of the high death-rate," and that "the local authorities ought chiefly to aim at the spreading out of the population," larger open spaces for circulation of air around each house, and the use of cheap railway transit to enable the artisan class to live beyond town limits in properly built dwellings. This radical cure seems also to be recommended by Drs. Parkes and Sanderson. In fact, nothing but demolition on a vast scale and proper reconstruction elsewhere, can meet this enormous and growing national evil.

We are glad to be able to cite in support of bold measures the recommendations of our Transatlantic cousins. In the Report of the New York Board of Health for 1868 appears the following paragraph (p. 10):

"Instead of removing from the crowded tenements the filth generated in and around them, it would be better for all to remove the over-crowded population to the neighbouring country, where cheaper and better dwellings could be procured, surrounded by plots of ground and supplied with pure air and ventilation. In the spaces about the houses, a cow or goat could be kept, to furnish food for the infants, and the general comfort and well-being of the residents would soon be so well established, that they would never consent to occupy unhealthy houses again. This cannot be done until means are provided by which the labouring population can return to their work expeditiously and at little expense. Steamboats, railroads, elevated or subterranean, to carry passengers quickly and cheaply through the city, deserve approbation and encouragement as the wisest of health measures."

Not that there is any "originality" or anything "truly American" in this project, as has been inadvertently alleged by a contemporary writer, nor that the Metropolitan Board of New York has really *done* anything in the matter. The paragraph

which we have quoted is nothing more than an epitome, albeit a good one, of proposals which have been long and repeatedly put forth by our own countrymen.¹ Charles Kingsley advocated the principle in a remarkable lecture at Bristol some years ago. And Mr. Simon, in his Eighth Annual Report, advised that local authorities should provide in the suburbs equivalent dwelling-space for those whom it was necessary to evict from condemned dwellings in the worst parts of towns. This then is one, and probably the most important, of those objects of sanitary administration which are seldom attainable within the limits of densely peopled districts, and which require either far larger areas of management than those proposed by the Sanitary Commission, or else powers of compulsory purchase of land outside these districts.

VI. On a general review of the alleged causes of disease and death in the various localities brought under the notice of the Royal Sanitary Commission, one is led to a few almost indisputable conclusions.

First, that overcrowding—or rather undue density of population—as we have just endeavoured to show, is the main evil to be encountered in the largest towns; and that, unless measures on a large scale are adopted to remedy this abuse, other efforts—however strenuous—of authorities—however intelligent—will only very partially benefit the public health.

Secondly, that the excessive mortality of our great centres of commerce and manufacture tells especially on the infant population; and so far is greatly aggravated by the absence of mothers in factories and workshops, and by the consequent neglect and maltreatment of their children. This was proved, conversely, by that marked reduction of infant mortality which occurred during the cotton famine in Lancashire, when the mothers were out of mill employ.

Thirdly, that the smoke and noxious vapour nuisances which so horribly deteriorate the atmosphere of manufacturing towns, though they may have been abated in various degrees in different places, will not and cannot be effectually dealt with by the present authorities, which represent those who produce the nuisances, unless compelled by a superior power, and this not necessarily a central board.

Fourthly, that the condition of the suburbs in great towns,

¹ These measures have been fully described and discussed by Dr. Rumsey in the following publications:—

1. 'Sanitary Legislation and Administration. London, 1858, pp. 27—32.

2. 'Homes for the Labouring Classes.' Reprint from 'Lankester's Journal of Social Science,' May, 1866, pp. 356—360.

3. "Address on Health," 'Soc. Sci. Assoc. at Birmingham,' 1868, pp. 14—18.

and of newly forming urban districts, so long as they are unregulated by an authority equal in power, intelligence, and scientific officering, to a good town council, must reproduce over a large surface of the country all the evils of the oldest and worst parts of towns.

Fifthly, that both in towns and villages, there is a serious deficiency of good drinking water, and an extensive and unjustifiable pollution of wells, brooks, rivers, and reservoirs; that this want of water, in towns especially, arises partly from causes which may be removed by better administration over larger areas, as river-basins, and partly from the want of power to take land beyond the limits of local-board jurisdictions.

The Sanitary Commission, therefore, wisely proposes that the local authority should be empowered to purchase—in certain cases by compulsion—land or rights for the purpose of water-supply, subject to the consent of a watershed authority, if and when such authority be created; otherwise, of the central authority.

Sixthly, that the existence in towns of slaughter-houses, knackers' yards, and other offensive trades, is authorized and protected by law to a needless extent, and to the serious injury of urban populations. We do not find that the Sanitary Commission proposes the enactment of any stringent regulations against these sources and hot-beds of zymotic disease, or any sufficient facilities for the removal of existing slaughter-houses and cowsheds from crowded populations. Yet such special regulations are necessary; and some superior authority ought to have the power of compelling not only the abolition of these establishments within the limits of urban districts of a certain population—say with 10,000 inhabitants—but also the purchase of land outside those districts for public abattoirs and cattle markets.

Seventhly, that beneficial as the recent statutory extension of the meaning of "nuisance" may have proved, the term might be made to apply to a yet larger number of influences dangerous to health, especially in the direction of offensive trades, badly constructed sewers and drains, water pollution, collections of kitchen refuse, &c. There is, by-the-by, a difficulty felt by several witnesses about pigs (478, 3218, 10,131). Is a pig, *per se*, a nuisance? Or does he only create a nuisance when kept badly and in uncleansed places? Can the consequent nuisance be effectively removed or prevented without abolishing the pig? We confess to a weakness for the animal which had more to do with the bone-and-muscle building of our Saxon ancestors than either beef or mutton.

If the preceding list of *gravamina* and *lædentia* is not so

complete as it might be, it points nevertheless to certain *corrigenda* and *desideranda* which are not, in our opinion, adequately, if at all, represented in the report of the Commission.

It seems to us marvellous that, in face of all the evidence contained in that report and in many other official publications, now forming a voluminous literature, the Commission should propose to retain the existing local-board districts, or even the parochial unions, as the future areas of sanitary administration, without some general combination of the smaller districts, and some re-adjustment or amendment of the boundaries of the larger. Equally strange does it seem that both are to be left, in the first instance, under the same local administrators, without the help of more competent assessors.

In a continuation of this article in the next number of the Review, we purpose to consider the scheme of organization proposed by the Commission, and the main provisions of Sir Charles Adderley's Bill, in connection with the Act just passed for the constitution of a Local Government Board. We shall also compare all these with the critical Report of the Joint Committee of the British Medical and Social Science Associations recently presented to those bodies.

IX.—Irish Poor-Law Medical Relief.¹

It has been very much the custom in England to think that matters are conducted in a very excellent manner. Every now and then all such self-laudatory ideas receive a rude shock; and unfortunately such interruptions to our national self satisfaction have of late become more frequent. The matter to which we now propose drawing attention is one for which England can lay small

¹ 1. *The Twenty-second Annual Report of the English Poor-Law Board*, 1869-70.

2. *Annual Report of the Commissioners for administering the Laws for the Relief of the Poor in Ireland*. Including the Twenty-fourth Report under the 10 and 11 Vic., c. 90; and the Nineteenth Report under the 14 and 15 Vic., c. 68, with Appendices. 1871.

3. *Return to the House of Commons obtained by Mr. W. H. Smith, M.P.*

4. *Returns by the Registrars-General—England, Ireland, and Scotland*.

5. *Reports of Proceedings of the English and of the Irish Poor-Law Medical Officers' Associations*, 1870 and 1871.

6. *Circulars published by the Irish Poor-Law Commissioners*, 1869, 1870, and 1871.

7. *Papers, &c., published in the 'Medical Times and Gazette,' 'Lancet,' 'British Medical Journal,' 'Medical Press and Circular,' 'Builder,' 'Times,' 'Pall Mall Gazette,' 'Saunders' News Letter,' 'Freeman's Journal,' &c. &c.*

claim for approbation. Indeed, the whole subject of the poor laws and of the medical relief of the necessitous needs very careful consideration.

We shall, in the present paper, advert to the relationship between medical poor relief and a diminished sick and death rate and reduced poor rates. The ancient maxim that the best governed state was that which took equal care of the humblest as of the most exalted citizens, truly represents what a poor law should be, for this should benefit not only the lowest class, but also the highest. But a vastly important question is intimately bound up with the administration of poor relief, namely, whether it should be carried out by a wholly governmental machinery or by popularly elected boards, subject to a supervising and paramount board, really representing the Government, or at least partly doing so.

We are bound to satisfy ourselves as fully as possible of the advantages and disadvantages of different modes of conducting poor relief and rating, and we need hesitate to overturn a system which, as a whole, has worked well in Ireland, because revision of it is loudly called for in England. Indeed, we cannot justify the increasing adoption in Ireland of the system of out-door relief, which has worked most injuriously in England, notwithstanding the favorable view taken by the Irish Commissioners of the matter. The encouragement now increasingly given to out-door relief in Ireland tends to foster vagrancy, although the Irish Commissioners in their report state their conviction that relatively to the population the number of vagrants in England far exceeds that of the same class in Ireland. Medical men, as well as all who have considered the subject, are well aware that out-door relief offers a strong temptation to the sick to remain in their own, commonly most unfit tenements, instead of entering suitable hospitals,—so liberally provided at the present time almost everywhere, especially in Ireland,—and so tends to render nugatory the best efforts and the oftentimes difficult work of the medical officer; as, for instance, the arrest of contagious disease or the prevention of the consequences of overcrowding.

Many other reasons could be adduced against encouraging the system of out-door relief which it is unnecessary here to touch upon, but our conviction is that its adoption by the Irish poor-law authorities is fraught with danger. This we say after considering the evidence now (July, 1871) being taken on the subject of union rating in Ireland before the House of Commons' Committee.

Concentration of action between the several branches of poor relief in Ireland has been productive of the happiest results; it

has lessened the amount of destitution immensely by securing the almost immediate medical care of every individual dependent on the rates, or liable to become so if unemployed.

Like the principle of life assurance, equitably carried out, Irish medical (poor) relief reassures the otherwise downcast spirits of the bread-winner, when overtaken with sickness, and destitute of resources. It is the same with his wife or children if ill; mutual confidence between the poor sick and the Irish poor-law physician is the general rule; the result is attention on the poor man's part to the medical officer's injunctions, and the furthering of hygiene amongst his family and friends.

Hence it happens that vaccination is so successfully carried out, and, in general, the separation of the sick from the healthy. The medical officer being in the great majority of cases also the registrar, is thereby materially aided in the furtherance of general vaccination. The Irish Commissioners always enjoin the necessity for the union of the offices of district registrar and medical officer. In a few cases, unfortunately, this arrangement has not been carried out, to the mutual disadvantage of the people and the medical officer. Notwithstanding the anomaly that the payment for vaccination is from fifty to one hundred and fifty per cent. higher in England than in Ireland, it redounds to the credit of the Irish poor-law medical staff that such successful results have been obtained in combating the present epidemic of smallpox. As for the argument that has been advanced by some, namely, that the exposure to contagion in Ireland has not been considerable, we need only observe that the disease has been constantly carried to various ports by sailors and soldiers, by domestic servants, and by persons of the artisan and labouring classes, arriving from places where the disease prevailed, and in many cases with the disease upon them. Such imported cases, indeed, have formed, if not the bulk of fatal cases, at least a very large proportion of them.

On the 13th July, 1871, the Irish Poor-Law Commissioners issued a circular to the Poor-Law Guardians on the subject of the smallpox epidemic as it appeared in Ireland during the preceding five months. Within that period 480 cases of smallpox were reported by the medical officers as having occurred in Ireland, chiefly in those ports most in communication with infected localities in Great Britain.

The number of deaths by smallpox in the present year (1871) in the City of Dublin prior to the 22nd July was fifteen; in 1870 the same authority—the 'Weekly Returns of the Registrar-General'—exhibits not a single death from this cause, and in 1869 but one, which took place in the person of a Swedish sailor, who died a short time after arrival in the port of Dublin,

the disease being fully out on him when he reached that place.

The history of smallpox in Ireland since the first operation of the Compulsory Vaccination Act supplies irrefragable testimony to the value of Jenner's discovery, notwithstanding the recent partial outbreaks of the disease in that country. Compulsory vaccination commenced in Ireland 1st January, 1864. The deaths from the disease that year in the whole island numbered 854. In 1865 they were 347; in 1866, 187; in 1867, 20; in 1868, 19; in 1869, 20; and in 1870, 32.

'The Irish Poor-Law Report' states—

"It is well known that an unusually severe epidemic of smallpox has prevailed in Great Britain for upwards of a year, and especially in London, Liverpool, and other large towns, to a very alarming extent.

"It is also generally known that the epidemic has reached Ireland, having very probably been introduced directly from England and Scotland by persons immigrating from thence with the disease, either declared or latent, but incubating at the time of their arrival here. In circulars on the subject of smallpox, addressed to the guardians on September 20th and December 21st, 1869, it was announced to them that no indigenous case of smallpox, and no death caused by it in any case originating in Ireland, had come to our knowledge through the returns of the Registrar-General, or the reports of the dispensary medical officers, from the end of June till the 30th of November of that year; and that the disease might therefore be considered to be extinct in Ireland during those five months. On the 30th of November, 1869, a dispensary case of smallpox was reported by the medical officer of the Maynooth dispensary district, and soon after (December 3rd) a case was admitted into the Belfast Union Workhouse Hospital, from which the present epidemic in part probably originated. The patient, Joseph Wilson, had recently come from Liverpool, where smallpox was then prevalent, to Dublin. The disease was developed within the period of incubation (ten to sixteen days), and he was admitted into the workhouse hospital on the 3rd of December, having probably left the seeds of the disease germinating among the persons with whom he had been in contact before his admission to hospital. No case of smallpox had occurred in Belfast for above four months before; the present epidemic had become plainly existent there in the beginning of March, 1870, and the disease has now shown itself along the whole eastern side of the country; nowhere, however, as yet, but by the occurrence of sporadic cases, except in Belfast and Drogheda, in which towns there have been numerous cases, and a considerable number of deaths. The Maynooth case above mentioned was traced to contagion caught in the Hardwicke Hospital, where the Swedish sailor mentioned in the circulars died of smallpox imported from abroad."

From a circular issued by the Irish Poor-Law Commissioners

¹ 31st March, 1871.

(13th July, 1871), we find that in the five months preceding that date 480 cases of smallpox were reported by the medical officers as having occurred in 53 Irish unions (that is, about one third of the whole number).

During the same period knowledge of 58 cases of importation of the disease from England, Scotland, or from other countries, was obtained, which, with the number previously recorded by the Irish Commissioners, gives a total of 92 imported cases. In consequence of the mild type of the cases generally, attributable to previous vaccination, a very small rate of mortality ensued.

The Commissioners infer, from facts in their possession, that the smallpox has been in this, as in previous instances, carried to Ireland by the ordinary intercourse between that country and Great Britain; and considering the number of different localities in Ireland into which it has been so imported, that it is surprising there are so few in which it has taken such hold as to establish a new centre of infection; in fact this has apparently happened only in Belfast, Dublin, Drogheda, and Wexford.

"These incidents are due in all probability to the comparatively good state of vaccination in the country, combined with other favorable sanitary conditions, the latter being partly due to the absence of any decided epidemic influences, and partly to the progressive operation of the sanitary laws."

From what we have ourselves learned, Ireland has suffered, though in immeasurably less degree than England, from infected persons above the age that exposed them to the operation of the Compulsory Vaccination Act; and it is to the credit of the Irish people themselves, as well as to the energy and influence of her medical men, that adults who had not previously been vaccinated submitted to that operation. Revaccination has also been very largely resorted to, but by no means so universally as its valuable protective powers entitled it to be. We would here observe that in Ireland revaccination is never performed with secondary lymph.

The whole history of the present epidemic of smallpox demands the consideration of the Government, or, if need be, of the Legislature, in connection with the absence of laws, or the defectiveness of such as exist, to restrict the arrival of infected persons, and of the ships or boats by which they are conveyed. In some places, it is true, persons have been prevented from landing, as we find in the following quotation from the 'Irish Report:'

"There is a vague impression on both coasts of the existence of some quarantine rule by which persons arriving on board ship with smallpox can be prevented from landing; and on the English coast the harbour authorities exercise this questionable power with

great rigour, as will be seen from the correspondence¹ regarding an Irish seaman prevented from landing at Maryport, who was obliged to make the return voyage to Bangor, in the County of Down, while suffering under a severe attack of smallpox in the fore-castle of a small collier. The only authority we have been able to discover for these proceedings is in the 52nd section of the Sanitary Act, 1866; but by that section the Privy Council is called on to make orders for treatment of the sufferers, as well as for preventing the spread of disease, but no such provisions appear to exist in the harbours of either country. In Ireland we insist upon the board of guardians of the union which contains the harbour, providing such hospital accommodation ashore as may be necessary for the treatment of persons arriving on board ship in smallpox.

"In Cork harbour the imperfect quarantine supposed to exist has caused landing to be prohibited, and a death followed this prohibition; and we have had much difficulty in overcoming the hesitation of the guardians of Cork union to perform their duty in regard to such cases under the Poor-Law and Medical Charities Acts, in consequence of vague notions regarding quarantine, and regarding the responsibility belonging to the nuisance authority, under the Sanitary Act, 1866. We believe, from what has occurred, that some alteration of the sanitary laws in this respect is urgently required in order to put an end to confusion and uncertainty."

We desire here to state our approval of the general effects of those sanitary exertions on the part of the Irish Poor-Law Commissioners and the Irish Poor-Law Medical Officers, aided so far as they have been by the guardians of the several unions; and we commend them to the consideration of official departments in England, presenting, as they do, something of unity of action in carrying out, first, early notification of disease, and, secondly, efficient preventive and curative measures. The steps taken, indeed, have been largely followed by happy results to the sick, to those threatened with sickness, and, in a very remarkable degree, have been a success in a financial point of view. The evidence afforded by the parliamentary returns made on the motion of Mr. W. H. Smith, M.P., and that given by the Secretary of the Irish Poor-Law Medical Association, and by the President of the English Poor-Law Medical Association, is eminently satisfactory on this matter. The total average mortality for England, Scotland, and Ireland, for the five years ending 1869, was respectively 1 in 43, in 41, and in 60 of the population; the average annual mortality from zymotic disease in the same order for each country was 1 in 190, in 194, and in 308. Again, deducting zymotic mortality, the relative death-rate has been 1 in 56, in 51, and in 78.

Looking at the more fatal forms of zymotic diseases, we find the following figures:

¹ See Appendix A, III, page 51, of the 'Irish Report.'

	Smallpox in 1868.	Measles.	Scarlet fever.	Fever.
England . . .	1 in 10,550	1 in 1861	1 in 990	1 in 1099
Scotland . . .	1 in 31,707	1 in 2365	1 in 1416	1 in 979
Ireland . . .	1 in 241,012	1 in 4526	1 in 2048	1 in 1594

The bearing that poor law medical relief expenditure has upon poor law taxation deserves the gravest and most attentive study by all classes of the community, whether statesmen, sanitarians, or simply ratepayers.

England.—Population, 21,649,377; medical relief, £272,000; total poor-rate, £7,498,059.

Scotland.—Population, 3,188,125; medical relief, £32,858; and total poor-rate, £863,202.

Ireland.—Population, 5,543,285; medical relief, £131,000; and total poor-rate, £829,521.

The relative prevalence of sickness and high poor rates in the three divisions of the empire are seen in the foregoing returns, and further reference to Mr. Smith's parliamentary paper shows the relative poor law taxation to be in the three countries—

	£	s.	d.	
England . . .	0	7	0 $\frac{3}{4}$	per head of population (1869—70)
Scotland . . .	0	5	7 $\frac{1}{2}$	„ „ (1868)
Ireland . . .	0	2	11 $\frac{3}{4}$	„ „ (1868)

It is some consolation to Irish ratepayers, on whom it is stated taxation, generally, presses more heavily than on the more favoured inhabitants of the sister countries, to find that both sickness and poor rates are lighter through the instrumentality of an efficient and systematic plan of poor law medical relief and registration of births and deaths.

Approval of the Irish system must not lead us to overlook certain defects attaching to it, which demand removal or amelioration. The medical officer is employed in at least a threefold capacity, viz., as physician, surgeon, and accoucheur, and is prevented from entering the service before twenty-three years of age; he holds also generally the office of district registrar, that of vaccinator, and that of sanitary medical officer. He is paid small fees for the duty of registration, a considerably smaller sum for his duty as vaccinator than that which his English brethren receive; his salary as medical officer averages £97 per year, but out of this he must provide himself with a horse and man. For his sanitary duties he is as yet wholly unpaid, unless at times of extraordinary prevalence of epidemic diseases, such as cholera. Furthermore, he is exposed to frequent worry and liability to actions at law as medical examiner of lunatics in his district, “without fee or reward.” It is true an effort is being made by Sir D. Corrigan, Bart., to remedy this obnoxious and most unjust state of things, and to secure to the medical officers

some reward for the performance of a duty onerous, and in many instances disagreeable.

We must not weary our readers too much, but it is necessary that another matter or two which affect the medical officer as well as the public should be carefully guarded against in England, in case of the introduction of a system of poor law medical relief after the Irish model, and should likewise be attended to in Ireland without delay. We refer to the indiscriminate issue of tickets which oblige the medical officer, on receipt of same at any hour of day or night, if presented to him in any place or left at his house, to immediately visit any sick person, if that sick person or the issuer of the ticket consider such visit required.

When we reflect that the persons authorised by the law, as it now stands, to issue such tickets are very commonly publicans, small dealers, and pawnbrokers, who are at almost all times only too willing to accommodate their customers; and that the famous red ticket is well nigh invariably sought by the husband or son or a friend, after return from labour in the evening, or when taking refreshment just before his neighbour the guardian, who possibly keeps the nearest public, shuts up for the night, we leave the reader to imagine the frequency of unreasonable calls to which the Irish medical officer of the poor law service is at present liable. Books of red or visit-tickets, better known as "scarlet runners," are kept ready signed at the houses or place of business of many guardians, so that they can be at once filled up by any member of the guardian's family or household, or, as happens in some cases, by the patient or his friend.

It is but just to demand that the issuing of tickets from all such irresponsible persons be withdrawn by law and that the duty be confined to the relieving officer or some suitable person, who should also be held responsible for any visit paid to a person able to pay for advice, and for cases of needless demands upon the doctor's time at night, &c. We have known tickets issued on a certain day being presented as urgent that night, though obtained only on condition of presentation the following morning. In such cases, and when the patient is able to pay for medical advice, as when possessed of property and in receipt of £2, £3, £4, or even more money per week, the payment of a reasonable fee to the doctor should be enforced jointly upon the issuer and the user of the ticket.

Retiring pensions have been rendered legal, permissively, to medical officers when incapacitated by age or sickness. It is a serious defect that the law is only permissive; at all events some remedy, by appeal, should rest with the medical officer who might be exposed to great annoyance, if not actually denied what should be his right. Cases will occur of officers

in whom anxiety on this point from some real or imagined pique by one or more guardians would, in the event of sickness, produce most serious effects. We recommend a reconsideration of this provision of the law to those in authority.

The registration of deaths and of sickness, framed on principles consistent with the strict province of the medical profession, could not fail to commend itself to the profession, and to be productive of great public benefit. The observations of Sir D. Corrigan and of others on the present requirements of the law demand attention. Dr. Burke, the Medical Superintendent of Statistics in the registration department, for Ireland, has given valuable information on the subject in his evidence before the Royal Sanitary Commission. He points out the difference existing between the forms used in England and Ireland in death registration, and considers that medical men should merely be called upon to state their opinion as to the cause of death. The fact of death, and the exact time of its occurrence, should be obtained from non-professional persons present at the time.

The deductions drawn from the working of the dispensary system in Ireland by the English Poor Law Board, in reference to the introduction of dispensary system into England, merit consideration in both countries. To some important points, treated of in the Report for England and Wales, we have already adverted. The fact that the relations between the public and the profession are not similar in some respects in the two countries is beyond question; in England and Wales the relative number of the average population to each medical practitioner is about 1000, whereas in Ireland the number is about 2500.¹

The free issue of tickets for attendance at the patients' homes in Ireland interferes to some extent with the practice of general practitioners; the delay and difficulty (as well as, in some cases, the risk of incurring annoyance from the guardian who issued the ticket) experienced by the medical officer in obtaining the cancelling of tickets given to persons able to pay for medical assistance, generally suffice to prohibit the overworked medical officer resorting to that mode of protection from an imposition alike upon him and upon the ratepayer. We are aware that some medical men hold the opinion that it would be better to permit a considerable proportion of the artisan class, as well as of the labouring classes in England, to obtain medical relief gratuitously at the cost of the State, sufficient remuneration being allowed the public medical officers, and all necessary medicines provided; in fact, to carry out by law a system

¹ 'Twenty-second Annual Report of the Poor-Law Board, 1869—70.' London, 1870.

which exists partly by law and too extensively by abuse of the legal provisions of the Irish poor relief act in Ireland.

Before the extension of the Irish dispensary system, since its amalgamation with the Poor Law Department, very many, if not all, of the small towns, and even some villages in Ireland, possessed educated apothecaries by whom the artisan and labouring classes were attended, and to whom many of the middle classes also resorted in ordinary illnesses; by them also the physician's and surgeons' prescriptions were compounded. The county infirmary surgeon and, in the larger towns, the consulting physician were had recourse to by all classes in cases requiring consultation; at the present time this state of matters is almost wholly changed. But few of the larger and scarcely one of the smaller towns now possess an apothecary; a difficulty has in this way arisen in many places in obtaining medicines, and as a consequence most of the country practitioners, who are, in the majority of cases, also the dispensary medical officers, are obliged to furnish their private patients with medicines. It remains to be seen whether the long-talked of pharmacy bill will meet the want now felt in some places without creating a new class of men who will not only sell medicines, compound the physician's and surgeons' prescriptions, but also proceed *ultra vires* to prescribe for the sick without the apprenticeship and knowledge required of the apothecary. But whatever minor disadvantages may have arisen, there is no question of the great benefit that has accrued to the public health by the operation of the dispensary system in Ireland; the medical profession should not, however, lose thereby.

We regret the erroneous statement quite lately made by a leading statesman¹ on the health of the people. Referring to the returns of the Registrar General of England we find, as our contemporary states, that we have no reason to congratulate ourselves on the state of the public health; and it is feared the enormous expenditure lavished on great drainage works has but intensified the agency for evil arising from modern sewerage arrangements, and the consequent pollution of rivers and other sources of water-supply. It is not a little singular that in Ireland, where as yet intercepting drainage,—as it is in many instances erroneously called, has not been in operation, the public health has been remarkably good. This year, however, the principle of enormous tunnels, containing vast volumes of decomposing sewage flowing, or rather stagnating, for dozens of miles through closely-packed habitations, has been approved by the sagacity of a parliamentary committee, instructed by the

¹ 'Lancet,' 12th August, 1871.

advocates of bricks and mortar, as the best system for Dublin, at the very time that the same principle has been condemned at Birmingham and elsewhere.

The question of the increasing pauperism in England and Wales demands careful investigation; it is well, therefore, so far as the comparatively satisfactory condition of Ireland in a sanitary aspect, as well as the comparative lightness of Irish poor rate, can furnish criteria towards arriving at an opinion, that steps are being taken to test the operation of a more extended dispensary system. Turning for a moment to the relative figures of England and Wales and Ireland, we see that in the former the annual value of property rated is £100,000,000, in the latter but £13,000,000 in round numbers. In England the number of paupers maintained out of the poor rate is 1,032,800, at a cost of over seven millions and a half sterling. In Ireland 69,384 paupers cost £668,202, and the medical charities' expenditure amounted to £129,936 more. Thus it appears that the relatively larger medical relief expenditure in Ireland coexists with a comparatively very much smaller total poor relief expenditure, as well as with a much lighter sick and death rate; in other words, whatever considerations may be adduced against sick relief on a large scale out of the public purse, most valuable results are obtained as regards the public health and diminished poor rates.

We would here claim for the very inadequately paid medical officers of Ireland a more suitable acknowledgment of their important services, and deliverance from the possible petty despotism of guardians clothed with a "little brief authority." Quite recently an attempt has been made to coerce medical men to certain peculiar obstetrical doctrines.

"An agitation," observes the 'Medical Press and Circular,' "which is fraught with the most serious considerations as regards the administration of the Irish medical charities, and the interests of those engaged in their working, has been set on foot within the last fortnight, in connection with the vacant office of Dispensary Medical Officer in Kingstown. A very lengthy manifesto has issued from the pen of an ecclesiastical dignitary, which propounds ideas alike repulsive to common sense and justice and to medical equality. The object of the agitation thus inaugurated is to show that no medical man should be allowed to take medical charge of a dispensary unless he is of the same religion as his patients, and as a necessary consequence that the whole of the medical charities of Ireland should be handed over to persons possessing one religion. It is sought to give a colour of reason to this monstrous dogma by arguing that craniotomy is an abomination to the Roman Catholic Church, and that, therefore, any one who might, could, would, or should, under any

circumstances, consider that operation advisable, is unfit to do obsteric duty amongst the Irish poor."

As we write, accounts reach us of the nearer approach of Asiatic cholera; we would endorse the general recommendation of the daily press, that no expense should be spared in endeavouring to check the march of that dread malady, and that sanitary inspection should not be carried out by police or other unqualified persons—

"Who are obliged to get their information at second hand from the dispensary doctors. In order to have the work efficiently performed it must be placed in skilled hands, who have the best means of knowing the circumstance of each case. The Act expressly provides for the payment for such skilled labour, and if it is not paid for it will not be efficiently performed," &c.

The writer quoted also speaks of the excellent system of sanitary police extending all over Ireland, under the Medical Charities Act; we have, however, already adverted to this subject, and do not return to it except to claim on behalf of the public the services of those gentlemen who have so recently conferred on Ireland the great benefit of efficiently combating smallpox, and who have also in past times exhibited equal energy and skill in dealing with cholera and fever.

The Irish Poor-Law Commissioners urged upon the Government the necessity for preventing the ingress of smallpox by medium of the persons or clothes of paupers removed from centres of contagion in England, as well as for the vaccination or revaccination of the crews of vessels trading between places in that country and Irish ports, as recommended by the Medical Inspector of Seamen for Dublin, and approved by the Dublin Local Marine Board. In reference to the former, Mr. Stansfeld stated, in reply to Mr. Downing, in the House of Commons, that the Government had advised the union authorities to suspend such removals during the prevalence of the epidemic. In answer to the latter recommendation, it was stated that the Board of Trade had no money¹ for the purpose. But surely the removal of paupers suffering from, or directly exposed to, cholera or fever, or other contagious disease, should be forbidden, just as the removal of diseased cattle is at times of cattle disease.

The whole subject of the removal of paupers who may have been born, or even have been resident in Ireland for a certain period, back to that country, when no longer able to labour for the benefit of the wealthier country, in which they may have spent

¹ We understand that a considerable sum of money is remitted annually from the receipts at the Port of Dublin to England.

forty, fifty, or even more years—the whole of their working days—demands patient and careful consideration. In this matter the working of the English Removal Act is blamed by the Irish authorities. The Court of Queen's Bench in England has declared it illegal to remove a wife and children to Ireland, when the head of the family, though absent, was known to be living. The highest legal authority in Scotland, however, refuses to abide by the English judicial decision. In all friendliness to our fellow subjects throughout the United Kingdom, we advise careful reflection on the following, which we transcribe from the 'Irish Report,' observing that that which is unjust can but reflect upon the nation permitting it; to our own knowledge, considerable numbers of English and Scotch of the working classes, and some, indeed, who once occupied a higher position, are in receipt of Irish poor-law relief.

"In England also this decision of the Court of Queen's Bench continues to be disregarded, even by the metropolitan police magistrates, and families deserted, or supposed to be deserted, are illegally removed to Ireland, although the husband and father is living, and ought himself to be included in the order, in which case a different birthplace would become the destination of the persons removed.

"A practice also appears to exist in London of illegally removing to Ireland persons who were not born there, but who happen to have lived there for three years; and a large and populous London parish, thronged with Irish residents, has thought it worth while to avail itself of a printed form of deposition and warrant adapted to such practice, and departing from the forms prescribed by statute."

In Liverpool, the report adds, a common practice is to seek relief in order to obtain a free passage to Ireland. The Scotch authorities continue to remove "maniacs, sometimes handcuffed, from Scottish lunatic asylums to Irish workhouses." The severance of husband from wife and parent from child has been reported on in previous years, and is still practised and vindicated by the "canny nation." The hardship of the proceedings of our northern neighbours is continually manifested by very untoward results, as instanced in the deaths of J. Hermon and her child, last year cruelly removed by the Parochial Board, Barony Parish, Glasgow, though both were suffering from disease, which proved rapidly fatal on their being sent as deck passengers to Ireland.

The plea advanced by Mr. Lumley, Assistant-Secretary to the English Poor-Law Board, that the abolition of the law of removal would deprive the Board of "one of the best remaining tests of pauperism," is thus met by the Irish Report:

"A test of destitution which operates practically as a denial of relief, however valuable in one sense it may be, in the absence of more appropriate tests, cannot be regarded with satisfaction as part of any benevolent system of poor law. The operation of such a test, when applied to cases of real distress, must often be attended by great suffering."

Many other matters connected with this important subject are ably dealt with in the Irish Report, details of which are given in the appendix.

"Ireland, no doubt, is fortunate in possessing a poor-law system founded on experience of the past, and such as to exclude, if well administered, the abuses of the old English system; and it seems to be worth considering whether a closer imitation of the Irish system might not prove that such a test as the removal warrant is as unnecessary in Great Britain as it certainly is in Ireland, and result, perhaps, eventually in a more thorough and efficient relief of the really destitute, in fewer cases of death by privation, an abatement of pauperism, and lastly, in an enormous saving of annual expenditure."

The subject of poor removal was under the consideration of Parliament in 1854, and was again brought under notice in the past session. The plea of want of accommodation formerly urged against an alteration of the system has not the same weight now, inasmuch as 11,000 additional beds have been provided in London alone within the last year or two. If poor removal was indispensable, it would not happen that Manchester and Bristol, Birmingham and Leeds, and no less than thirty-three English counties, should practically have been able to do without it; for returns show that very few have been removed within the last three or four years from those towns and counties, though large bodies of Irish are resident in several of them.

In Ireland the decrease of the population is in some respects unsatisfactory. The population formerly being largely agricultural and dependent upon small wages, were the more willing to serve in the army in time of war; on the other hand, being badly fed and housed, they were liable to severe epidemics. At present the price of labour is immensely enhanced; Irish manufactures and trade have increased, notwithstanding the powerful rivalry and great capital of England and other countries. The great rise in the price of meat offers a premium for Irish grazing land and green-crop tillage, the island being for the most part better suited for such than for raising cereals. The very great demand for labour on the American Continent and in the colonies is a potent cause of reduction in the numbers of the Irish.

The diminution of able-bodied adults in the Irish work-houses, the comparative harmlessness with which the epidemic wave of variola has broken upon the Irish shore, the diminution of fever, the growing confidence of the people in the efficacy of preventive as well as curative and restorative medicine, as carried out by the Irish poor-law system, coupled with the self-denying energy of its medical staff, are matters of congratulation as well for Ireland as for the sister countries. In England the melancholy fact of increasing pauperism, a decreasing health-rate, the fatality of variola attributable in degree, like the growing severity of bowel affections and certain zymotic exanthemata, and other forms of disease, in the minds of many, to vast schemes of sewerage, require serious consideration and renewed energy. But there is no need for despondency, provided parliament is not any longer found wanting in sanitary legislation on a wide, and judicious, and energetic system, and that shall not be worked on the "permissive" principle.

Bibliographical Record.

West on Nervous Disorders of Childhood.—The College of Physicians has been exceptionally fortunate in the lectures delivered this year. Each course has presented merits of a special kind. One displayed power of scientific research of the highest order, another extensive erudition, while the Lumleian Lectures before us are remarkable as the happy expression of the matured wisdom of a Nestor in our ranks. Dr. West conferred a favour on the profession in selecting the nervous disorders of childhood as his subject; on it no one could speak with more authority, and few so well.

After a short sketch of the history of the foundation of the Lumleian Lectures, the lecturer enters on his task. He arranges the nervous disorders of childhood with reference to (1) sensation, (2) motion, (3) speech, and (4) mental and moral powers. In the first lecture neuralgia, convulsions, and epilepsy, are considered. After some remarks on the first two disorders, the mode of origin of epilepsy and the elements of prognosis to be gathered from the history and peculiarities of each form are admirably portrayed. The observations on treatment are scanty, if we except some valuable passages on the influence of moral control.

Chorea has been handled with loving care, and to it the lecturer has devoted more space than to any other disorder. The greater frequency of the disease in the female (64 out of 100 cases) is not solved, he shows, by the common explanation of the greater importance of the changes of puberty in the female, for the preponderance of female cases is as great under five years of age as later. The greater tendency of girls to convulsive affections than boys is no better as a solution, as is shown by figures collected by the author and other observers. With the sure caution of maturity, Dr. West says—

“We may, therefore, take the fact, I do not attempt its explanation, that as with acute hydrocephalus and all the graver forms of disease of the nervous system, whether acute or chronic, the liability

¹ *On some Disorders of the Nervous System in Childhood; being the Lumleian Lectures delivered at the Royal College of Physicians of London in March, 1871.* By CHARLES WEST, M.D., Fellow and Senior Censor of the College, Physician to the Hospital for Sick Children. London, 1871.

of the male sex preponderates; so all through life the slighter forms prevail in the female sex. Neuralgia, chorea, hysteria, are the special heritage of woman from her earliest years, as much as the gentle voice, the flowing hair, the graceful form."

The theories which refer chorea to one set of causes are shown to be equally baseless, and we heartily endorse the arguments urged against the somewhat fashionable hypothesis of embolism of the small vessels of the corpus striatum. In the passages on heart disease and rheumatism, in connection with chorea, there are some very suggestive observations, and a very noteworthy case of dilatation of the heart is recorded.

In speaking of the treatment of this disorder the cases in which moral and mental culture are useless are first pointed out. The advantages of restraining the movements in some cases are next referred to, and then we have the author's estimate of drugs. He has no belief in sedatives (including chloral and bromide of potassium), except as producers of sleep, and has never seen the slightest benefit from the use of antispasmodics. Sulphate of zinc is spoken of as the only remedy approaching a specific, and the use of large doses of tartar emetic, as much as nine grains in one day, is noticed with favour. Chalybeates and purgatives are mentioned as occasionally useful, but oddly enough no mention whatever is made of arsenic, a remedy which has in our hands proved second to none. The remainder of the lecture is given to the consideration of the various forms of paralysis, and a new variety, the paralysis associated with rickets, is described. In the last lecture there are some important observations on aphasia as it occurs in the young, which are followed by a review of the mental and moral peculiarities of childhood. Such are the contents of this little book. We have no space to analyse them further, nor can we convey to our readers any adequate notion of their value. They are the results of a life of observation, and touch mostly upon the obscure parts of the subject, on which the author's long experience has enabled him to give us new and useful information. The book has given us much pleasure, and will repay perusal. If for nothing else, it is most valuable for the simple and beautiful expression of the *religio medici*, which runs all through the lectures, like a vein of bright metal in the darker ore. In these days of unstable belief this manly and honest confession of faith by an honoured veteran will, we trust, not be uttered in vain.

Dr. Brown's *Analytical Chemical Tables*.¹—We fail to see why this expensive little pamphlet of twenty-four pages has been published. Dr. Odling's 'Manual of Practical Chemistry' not only

¹ *Analytical Tables for Students of Practical Chemistry*. By J. C. BROWN, D.Sc. London, 1871.

covers the whole ground occupied by the tables published by Dr. Brown, but it gives besides these further explanations and descriptions without which tables become the mere instrument of cramming for examinations. The present pamphlet, moreover, though it costs half-a-crown, is filled with scattered letter-press which might have been greatly compressed, say into twelve pages, without disadvantage. It is equivalent to the analytical appendix found in all works of practical chemistry in use in medical schools. With every wish to recognise the merit of any new book which is likely to further the progress of chemical knowledge or increase the efficiency with which it may be taught, we fail to discover any special characteristic of novelty or worth in the pages before us, unless it be the frightful name *canuose* assigned by Dr. Brown to cane sugar, and a scheme for the separation of those organic alkaloids which do not occur together. The multiplication of little fragmentary laboratory helps is an unfortunate feature of the present era of scientific instruction.

Liverpool Medical Reports.¹—That published “Hospital Reports” have multiplied in a marvellous manner during the last few years is a circumstance which admits of no questioning; but whether such multiplication has been the result of a *felt* necessity for such works admits, we think, of something more than doubt; indeed, we are led, from the following circumstance, to demur at once to any such necessity existing, viz. that with one, or at most two exceptions, the circulation of these “Reports” is so limited as to really merit the term insignificant. Why, then, it may be asked, *are* they published?

Without venturing to give our own special opinion upon this particular point—a point which could, undoubtedly, be best answered by those who are contributors to such publications—it is not difficult to perceive that any good they may do, or be supposed to do, is more than counterbalanced by their tendency to drain away, as it were, from our ordinary medical journals a good deal of that particular kind of information which, hitherto, has been found in their columns.

For instance, in the present volume of the ‘Liverpool Medical and Surgical Reports’ we find that a goodly portion of the book consists of a periscope of medicine, of surgery, of midwifery, and abstracts of the proceedings of various medical institutions and societies in Liverpool. These we certainly think would find a more fitting place in the pages of some of our well-known and long-standing weekly, monthly, or quarterly journals, rather than in an annual volume of ‘Medical and Surgical Reports,’ where they impart such

¹ *The Liverpool Medical and Surgical Reports.* Vol. 4. October, 1870. Edited by P. M. BRAIDWOOD, M.D., and REGINALD HARRISON, F.R.C.S.

a motley character to the work that attention is of necessity attracted to it, and comment rendered inevitable.

In the case of the "Reports" we are now noticing, it may be said that the book does not profess to emanate from any particular hospital, but to embrace the professional proceedings of *all* the medical and surgical institutions in Liverpool; but even admitting this, it seems to us that, excluding the original communications, the remainder of this volume might with greater propriety have found a resting place, with other similar matter, in the pages of 'The Medical Times and Gazette,' 'The Lancet,' and the like periodicals.

Apart from the objection which we have made above, it gives us unfeigned pleasure to be able to speak in terms of approbation of the few original articles contained in the reports. These are eleven in number, and, as a whole, possess considerable merit; but it would be too much to expect, or to assert, that they are all equally good.

Dr. Waters offers some "Observations on the Treatment of Pneumonia;" Mr. H. Lowndes writes "On Delayed Union of Fractures;" Dr. Wallace "On Thoracentesis," with "an illustration;" Dr. J. C. Brown gives a "Table for the Examination of Urinary Calculi;" Dr. Braidwood's communication is "On Cancerous Degeneration of the Kidney in Children;" Dr. Turnbull discourses on "Recent Advances in the Prevention and Treatment of Phthisis;" Dr. Edgar A. Browne gives "Detached Notes on Eczema;" Dr. Rogers and Dr. J. C. Brown produce a joint communication "On Fractured Ribs in Insane Patients, with Report and Analysis;" Mr. Bickersteth has "Notes of Cases treated upon Antiseptic Principles;" Mr. Harrison contributes "Illustrations in Operative Surgery" (illustrated); and T. S. Walker concludes the list with an article "On the Treatment of Ulcers of the Cornea and Nebulæ."

Looking at the practical character of the various articles and the reputation of the writers, it will be at once imagined that the volume has, so far as these articles go, an innate merit; but then, whilst the whole book consists of 191 pages, only 118 of the number suffice for what we contend is the legitimate scope of the work, the remainder—getting on for one half—being occupied, as we have before said, with matter which it would have been better to omit.

The few illustrations in the book are excellent, and the two given with Mr. Harrison's straightforward and clearly written paper are as good as it is possible, we imagine, to make them.

Dr. Tilt on Change of Life.¹—In our number for April, 1857, we had the pleasure of introducing to our readers the second edition of

¹ *The Change of Life in Health and Disease: a Practical Treatise on the Nervous and other Affections incidental to Women at the Decline of Life.* By EDWARD J. TILT, M.D., &c. Third Edition. London, 1870. Pp. 296.

Dr. Tilt's book on the 'Change of Life in Health and Disease,' and remarked that he was doing much good to the profession and the public, in striving (and in a great measure successfully) to remove the prevalence of erroneous views then existing, and to engage the attention of medical men more earnestly to the important subject he was dealing with. We believe that much more attention has since been paid to the pathology and general treatment of the various diseases which may occur at that eventful period of female life, and are glad to find that he still presses upon us the necessity for further investigation by the publication of a third edition. Dr. Tilt has considerably enlarged the volume recently published, and, although keeping much to his original plan, has nevertheless introduced physiological views and ideas which will have to bear the test of critical examination by men equally ardent in the cause before they can be accepted without reserve by the profession, or be made useful in a practical point of view.

The chapter on the "Diseases of the Reproductive Organs" has been greatly increased, and Dr. Tilt calls especial attention to his views of the physiology and pathology of the ganglionic nervous system, challenging criticism "to the utmost" of this chapter, for the sake of doing good service to himself as well as to science.

It is impossible for us, in a short notice of the work, to do justice to his opinions and arguments with reference to what he calls *gangliopathy*, by which he means a condition in which more or less debility is associated with paralysis, hyperæsthesia or dysæsthesia of the solar plexus and the central ganglia of the sympathetic system. We shall, therefore, leave it to those who will take the pains to study what he has said upon the subject, to accept or reject the views he has enunciated, a task not rendered more easy by a certain discursiveness in his method of treating the subject and the frequent interruption occasioned by quoting the opinions of others. No doubt truth will in the end prevail, and we must feel obliged to Dr. Tilt for bringing his views prominently before the medical public. With the following quotation from his preface we entirely agree, and of the practical utility of his book (would it were in a more condensed form) there can be but little doubt. He says—

"When the last census was taken in 1861, 1,177,535 of our fellow-countrywomen of the age of 45 and under 55 were living in Great Britain and Ireland, and I can scarcely exaggerate the importance of a work which tells their history, records the probabilities and the inevitabilities of their future, and investigates the many diseases by which it may be chequered. This volume professes to do so, and it is founded on the tabulated estimates of the symptoms and of the diseases of 500 women who were at the change of life, or who had passed the *ménopause*."

We applaud Dr. Tilt's energy and continued industry, and cordially recommend his new edition to the profession.

Drs. Madden's and Meadows' Manuals of Midwifery.¹—These two volumes are both enlarged editions of former works. Neither of them professes to be much more than a practical guide to such students and practitioners as like to have their midwifery made easy, and are disinclined to the labour of mastering more ambitious treatises. Of their class they are both excellent.

Dr. Madden's is an enlargement of a little work by Dr. Maunsell, which, under the title of 'The Dublin Practice of Midwifery,' has enjoyed a wide reputation in Ireland. It is especially interesting, as the directions for contending with certain difficulties show the practice of the present day in the Irish metropolis in very favorable contrast with what it used to be in former years. Of old, unhappily, the perforator and crotchet were favorite instruments, and were unquestionably used in many cases in which modern practitioners would never dream of employing them. Dr. Madden seems to us to have run into the opposite extreme, and to have such a dread of the operation that he passes it over in a hurry, as if it was something hardly to be spoken of. It would be no easy task to say, from his description, what are, in his mind, the indications for its performance.

Dr. Meadows' work is more ambitious in its arrangement. It enters much more completely into the details of the subject, and it professes to be—as, indeed, it is—a tolerably complete handbook of the science and art of midwifery. It is concisely and clearly written, and the information is, on the whole, on a level with the most recent knowledge of the day.

There are certain omissions in both, however, which have struck us, and are to be regretted, and in some points both authors adhere to theories which recent researches have shown to be more than doubtful. Thus, we are surprised to find Dr. Madden teaching, with regard to the decidua, views which may be fairly called exploded. He talks of it as resembling a layer of coagulable lymph, and does not seem to be aware that it is, in fact, the thickened and hypertrophied mucous lining of the uterus. Again, he tells us that the decidua reflexa is formed by the ovum pushing before it a fold of the decidua vera in its descent into the uterus, a view which is, to say the least of it, very antiquated. On these points Dr. Meadows has much more accurately given the teachings of modern science.

Then, again, with regard to the gravid uterus, both authors talk of the cervix shortening during pregnancy—Dr. Madden as if it was an admitted fact, Dr. Meadows quoting a certain Dr. Taylor, of

¹ 1. *The Dublin Practice of Midwifery.* By HENRY MAUNSELL, M.D. New Edition, revised and enlarged, by THOMAS MORE MADDEN, M.D., M.R.I.A. London, 1871.

2. *A Manual of Midwifery.* By ALFRED MEADOWS, M.D. Lond. The Second Edition, revised and enlarged. London, 1871.

America, as throwing some doubt on it, neither of them apparently being aware that Dr. Matthews Duncan has proved to demonstration that no such shortening occurs.

Again, with reference to the mechanism of parturition, both authors adopt Naegelé's views, and make no reference to the writings of Leishman, Duncan, Ritchie, and others, who have proved pretty conclusively that the usual description of the cranium entering the pelvis in the first position, with the right parietal bone on a lower level than the left, is quite inaccurate.

Thrombosis and embolism in the puerperal state, and the occasionally dreadfully sudden deaths resulting from them, are not even mentioned by either author.

In their descriptions of the forceps both authors recommend the blades to be applied in relation to the child's head, and over the ears. Barnes has well shown how fallacious are the doctrines of the schools on this point, and how, in fact, the forceps are usually passed within the ilia. We should have liked to have seen some reference to his valuable observations on this topic. Dr. Meadows' chapter on the cephalotribe is especially to be commended as containing a most judicious estimate of the uses of that instrument.

A serious omission by both authors is in not describing Credé's method of managing the third stage of labour by firm uterine pressure, and by thus squeezing out the placenta. We look upon this as one of the greatest improvements in modern midwifery, and it should certainly have been noticed. By following this plan, not only is the patient effectually secured from the risk of hæmorrhage, but the uterus itself is made, in nine cases out of ten, to expel the placenta within two or three minutes of the birth of the child. Having invariably followed this method for several years, we can speak strongly of its efficacy and perfect safety. We are the more surprised at this omission on Dr. Meadows' part, as we observe that he notices with favour the application of uterine pressure in certain cases of labour delayed by defective uterine action, after the method described by Kristeller under the name of "*expressio fœtus*," and more recently advocated in this country by Playfair.

We have thus ventured to point out a few omissions in these otherwise valuable little works, which, we doubt not, will find many readers.

Tilbury Fox on Eczema.¹—This little work contains three lectures delivered before the Medical Society of London, and they are preceded by an introductory chapter and followed by a summary.

¹ *Eczema: its Nature and Treatment; and, incidentally, the Influence of Constitutional Conditions on Skin Diseases.* Being the Lettsomian Lectures for the Session 1869-70. By TILBURY FOX, M.D. Lond., M.R.C.P. London, 1870. Pp. 68.

They appear to be printed just as they were delivered, even the conventional language of the lecture-table being preserved without alteration. Written as the Lettsomian lectures generally are, with the view of giving to the busy medical practitioner a practical account of some common class of diseases, Dr. Fox's discourses omit all arguments of a merely theoretical nature, and dwell chiefly upon the visible phenomena and clinical varieties of eczema, and on the best methods of treating them in their manifold aspects. In the first lecture it is shown that Willan's views of eczema have been very much misrepresented, for that author did not lay any special stress on the initial lesion or vesiculation of the disease as being a typical feature, but rather on the fluid which the vesicles contain, and which is really the essential character, for although eczema may pass through the stages of papulation, vesication, pustulation, and squamation, the presence of a limpid fluid at some period can always be ascertained, and without such a fluid eczema cannot be said to exist. In the second lecture Dr. Fox treats of the morbid anatomy of the disease, which he believes to consist of a perverted innervation, and not to be due to a special blood crasis; in fact, he holds that eczema is a "catarrh" of the skin, having a close analogy with catarrh of the mucous membranes, allowing for difference of concomitants. It is also materially influenced or modified in its characters by varying constitutional conditions, such as gout, syphilis, and the nervous and strumous diatheses, and also by concomitant morbid affections in internal organs, as the bronchial tubes and the intestinal tract. In the third lecture the treatment is fully discussed, and it is shown that there is no one remedy for eczema, but that it must be treated in accordance with the stage at which it has arrived, the peculiar features it presents, and the constitutional habit of the patient. In the earlier stages soothing remedies should be freely used, in order to allow the eczema to pass quickly to the stage of squamation, when local curative means are most efficacious. In the cases presenting a gouty or strumous or other constitutional tendency, the remedies appropriate to those conditions must be employed; but Dr. Fox urges in general, in the first instance, a pretty free internal use of antiphlogistics, antimonial, and diuretics. In the chronic stages simple astringents and stimulants should be locally applied, and tarry remedies should be used when there is much itching, and the soap treatment, as recommended by Hebra, should be adopted when there is much infiltration.

These lectures contain, in a condensed form, much useful information, and they constitute a compendious monograph on an important class of cutaneous affections, which, it may be observed, includes not only the eczema as it is commonly defined, but also such maladies as some forms of so-called porrigo, impetigo, and psoriasis.

Spencer Watson on Tumours of the Orbit.¹—In this part the author treats of the solid tumours of the orbit, arranged in the order of classification of Virchow, which in the former part was not followed, considering that the fluid and semifluid tumours, its subject matter, might otherwise be better elucidated in a diagnostic point of view. But they may very well be considered in an arbitrary method of arrangement, as they all, with one or two exceptions, belong to Virchow's first two groups. Fibrous tumours of the orbit, however rare, still, Mr. Watson considers, should rightly be made to include some of those that used to be called *scatoma*; and of the orbital fatty tumours—he remarks on the close and hitherto unsuspected relations they bear to goitrous exophthalmia, under which head he quotes Demarquay—should probably be classed some of the old-reported cases of lipomatous or fatty growth.

Of glioma, the author doubts the appropriateness of the name—"at any rate, so far as the retina is concerned"—in which the neuroglia form so large a portion of the thickness of the nervous expansion. "The thickness of the granular layers, which are composed almost exclusively of the so-called neuroglia, is perhaps equal to the rest of the retina, and is now placed in the interstices of the true nervous tissue, so as to support and connect them, but lies outside these in distinct layers." He adopts the rule by which an eyeball thus affected, and as much of the intra-orbital portion of the optic nerve as possible, is to be excised at once, "provided only there be decided *growth*," however slow their advance may seem to be. The chloride of zinc paste is to be applied subsequently to the extirpation. The diagnosis in the early stages of the disease is the point of difficulty left to us. Sarcoma (originating generally in the choroid) is the other main division of the solid tumours, not truly cancerous, and the last here considered. Some cases are called, for convenience sake, *glio-sarcomatous*. It is much to be regretted that even now, with the ophthalmoscope and all other modern aids to science, the tumours of the orbit are still so hardly to be defined.

Barnes' Obstetric Operations.²—No lengthy notice is required of a work which has already been fully reviewed in our columns. The fact that a second edition has already been called for is the best proof that the sterling merits of the work have received their reward in the appreciation of the public. Dr. Barnes has done his best to make the second edition not a mere reprint. Several important topics, not dwelt on in the former volume, have special chapters devoted to them, such as the various deformities of the pelvis; obstructed labour from the presence of tumours; ruptures of the

¹ *On Abscess and Tumours of the Orbit.* Part II. By SPENCER WATSON, F.R.C.S., &c.

² DR. BARNES' *Lectures on Obstetric Operations.* Second Edition.

uterus and the vagina; and these have been written on in the same clear and practical style as the rest of the work. Among these additions the chapter on ruptures of the uterus is especially worthy of notice. The causes and symptoms are very clearly explained, and form the best description of this terrible accident we possess. We are glad to find Dr. Barnes distinctly recommending abdominal section when the fœtus has escaped into the abdominal cavity. The results following this treatment, especially in America, are far more favorable than we could dare to hope in the presence of such a complication, and it is to be regretted that this is hardly yet recognised as the proper course to pursue in these cases.

As a whole, Dr. Barnes' book is a model work, which cannot fail to ensure its author a lasting place in the roll of British obstetricians.

Oppolzer's Lectures on Pathology.¹—Not having the honour of the personal acquaintance of Dr. Stoffella, the editor of Oppolzer's lectures, we are consequently ignorant of the habits and constitutional peculiarities of that physician. But judging from the length of time occupied in placing Oppolzer's lectures (prepared, as they were, long ere he entered on his editorial task) before the profession, the inference arises, that after the publication of each successive part he has sunk outworn into a state of lethargy and torpor; or that, after reviewing such a monument of his own industry, he has found it necessary to subdue excitement by assiduous devotion to the pipe, prolonged until mental serenity has been restored.

But whichever surmise be true, or whether some other cause of delay exists, some steps should be taken to hasten the appearance of these lectures, if the editor be in earnest in wishing to give permanence to Oppolzer's oral instruction. The original advertisement of the work stated that it would be published in eight parts. Of these only four, occupied entirely with the pathology of heart and lung diseases, have as yet appeared, and the production of these four, completing the first volume of 778 pages, has been the labour of five years.

There is no doubt as to the value of Oppolzer's teaching; but at the present rate of publication Oppolzer himself, and many of the subscribers to the work, are likely to be in their graves before that teaching appears in print complete.

Since this was written the profession has to mourn the decease of the great German teacher, and regret will be felt that for the future lectures that Dr. Stoffella may place before the world—should death not overtake him in his tortoise-like rate of progress—the revision of the “master” cannot be had.

¹ *Oppolzer's Vorlesungen über specielle Pathologie und Therapie bearbeitet und herausgegeben, von Dr. EMIL RITTER VON STOFFELLA. Erster Band. Vierte Lieferung. Erlangen, 1870.*

Weightman's Medical Practitioner's Legal Guide.¹—This is a well-conceived and well-executed undertaking, calculated to be of much use to medical men, who, as a class standing in numerous and special relations to the State, are very frequently brought face to face with the law in its operations with regard to themselves, their patients, public bodies, or the public at large. A volume in which the laws relating to the medical profession are brought together, placed in a clear light, and illustrated by reference to cases decided in the law courts, whereby their right interpretation is fixed, cannot fail to be appreciated by medical men above all others, although such a digest must also be found useful to all concerned in dealing with those laws.

Besides the recital of the provisions of the statutes in question, Mr. Weightman enters on various matters of interest to professional men *quoad* their profession and its relations to society. For instance, he has dissertations on the ancient and on the modern orders of the profession; on their rights, privileges, immunities, liabilities, and disabilities; on medical ethics and medical education, and on medical titles.

The laws considered in their bearing upon the profession are—those of copyright; the anatomy act; the law of partnership; the law of life assurance; the public health acts; the lunacy acts; coroner law; the laws for registration of deaths, &c.; the poor-law legislation, and the recent legislation for regulating the qualifications and status of chemists and druggists, and of dentists.

The general review of the objects and intents of the laws relating to the profession is followed by the second and larger section of the work, devoted to the recital of the clauses of those several laws, among which the lunacy laws form the principal figure by their redundancy and elaboration. And yet all the copiousness of the legislative enactments relative to lunatics and those who are directly or indirectly concerned with them, as set forth by Mr. Weightman, represents only a part of the provisions enforced; for they are supplemented by much commissioner-made law, variable in quantity and in kind, and, like the burdens of the scribes and pharisees of old, heavy and grievous to be borne. However, Mr. Weightman is not to be blamed for the omission of these accretions to the law, which, if exuberant in respect to the laws regulating the care and treatment of lunatics, are also not deficient in connection with the administration of other acts of parliament.

To conclude this brief notice, we have much pleasure in recommending the treatise to our readers at large as a necessary appanage to their libraries.

¹ *The Medical Practitioner's Legal Guide; or, the Laws relating to the Medical Profession.* By HUGH WEIGHTMAN, M.A., Barrister-at-Law, &c. London, 1870. Pp. 416.

Bacon on Writing of the Insane.¹—This brochure is, as the author tells us, “intended merely to furnish illustrations of the writing of the insane, and not to support any particular views.” It presents fac-similes of the writing of several patients, of whose madness these productions afford evidence enough. They furnish pictures of insane minds painted by themselves. Our own impression is that insanity shows itself, on the whole, even more patently when the ideas of its victims are put into writing than when conveyed by words or actions. There are some cases in which the aberration may not attract attention in conversation, but will at once reveal itself in the manner and style of writing and composition.

The author gives examples of insane writing “arranged under the following heads:—1, as illustrating chronic insanity; 2, as illustrating acute attacks; 3, as (rarely) the sole evidence; 4, as a sign of convalescence; 5, as indicating an on-coming attack; 6, as illustrating the phases of cases of ordinary mania; and, 7, as showing the changes the handwriting undergoes in general paralysis.”

Dr. Bacon’s contribution will be read with profit by all who are making insanity a study.

Thorowgood on Asthma.²—This small work the author warns us against regarding as a systematic treatise, and at the same time challenges for it a clinical character, as being based upon one hundred and sixty cases that have come under his observation during the last six years, although he, in fact, adduces but twelve of them in illustration. Our impression of it is that it is as much a systematic as a clinical treatise on asthma, and is to be accepted as a brief exposition of the general phenomena and principles of treatment of asthma, especially of that variety known as spasmodic asthma. As such it may be read with profit by those who have not opportunity to study the larger work of Dr. Hyde Salter.

Dr. Thorowgood recognises an asthmatic condition as dependent either on a contractile spasm of the muscles which encircle the smaller bronchi, or on a paralytic condition of those muscles, accompanied by more or less loss of diaphragmatic power. He further divides spasmodic asthma according to its exciting causes, into hæmic, reflex, and specific.

Having dealt with the symptoms, general pathology, and treatment of dry and moist spasmodic asthma, he treats of the effects of the malady on the lungs, heart, and system generally, and then devotes a chapter to the characters and treatment of bronchitic

¹ *On the Writing of the Insane; with Illustrations.* By G. MACKENZIE BACON, M.D., &c. London, 1870. Pp. 24.

² *Notes on Asthma; its Nature, Forms, and Treatment.* By JOHN C. THOROWGOOD, M.D., &c. London, 1870. Pp. 134.

asthma, and brings the work to a conclusion by a chapter on hay-asthma, or summer catarrh.

The reader of this small treatise, although failing to discover any new or recondite teaching in pathology, may meet in its pages with some useful practical hints, some judicious notes on treatment, and a sufficient outline of the subject to refresh his memory.

Black on Examination of Urine.¹—This essay is a reprint from the 'Transactions of the St. Andrew's Medical Graduates' Association,' and is, therefore, well known to some of our readers. The author has not pursued the customary course of recapitulating the well-known chemical relations of the urine in disease and the tests to be employed in examining them, but has done better service in noting the modifications of the physical characters of the secretion—the quantity, density, colour and the aëribility, and the relations of urinary sediments in connection with evident lesions affecting the body, generally or locally.

Another singularity in this paper on the examination of the urine is the absence of microscopical detail, Dr. Black having kept himself closely to narrating those qualities of the renal secretion which address themselves to the unaided senses, and to examining their meaning in reference to diseased action wherever found.

Eliot and Storer on Inorganic Chemistry.²—We ought to have noticed this work earlier. We may here content ourselves with saying that its lessons in manipulation are written in an easy and interesting style, yet are distinguished by singular minuteness and accuracy. In the preparation of the theoretical as well as experimental illustrations of chemical lectures this book will prove of great use to teachers.

Recent Works on Diseases of Children.³—The press has of late been prolific in the production of works on diseases of children, but all those now before us are new editions of treatises well known

¹ *Clinical Examination of the Urine in relation to Disease.* By C. BLACK, M.D., &c. 1870.

² *A Manual of Inorganic Chemistry, arranged to facilitate the Experimental Demonstration of the Facts and Principles of the Science.* By C. W. ELIOT and F. H. STORER. Second Edition. London, 1868. Pp. xiv, 605, and lix.

³ 1. *A Practical Treatise on the Diseases of Infancy and Childhood.* By T. H. TANNER, M.D., &c. The Second Edition, revised and enlarged by ALFRED MEADOWS, M.D., &c. London, 1870, pp. 472.

2. *The Diseases of Children.* By FLEETWOOD CHURCHILL, M.D., and FLEETWOOD CHURCHILL, Jun. Third Edition. London, 1871, pp. 900.

3. *A Practical Treatise on the Diseases of Children.* By J. F. MEIGS, M.D., and W. PEPPER, M.D. Fourth Edition. Philadelphia, 1870, pp. 921.

4. *The Management of Infancy, Physiological and Moral; intended chiefly for the Use of Parents.* By ANDREW COMBE, M.D. Tenth Edition. Revised and edited by Sir JAMES CLARK, Bart., M.D. Edinburgh, 1870, pp. 254.

5. *The Wasting Diseases of Infants and Children.* By EUSTACE SMITH, M.D. Second Edition, revised and enlarged. London, 1870, pp. 309.

and appreciated by professional men ; also, having been reviewed heretofore, they now, consequently, demand no lengthened notice of their character and contents. Moreover, a *résumé* of the present state of knowledge and opinion respecting their subject matter, which their appearance might have suggested as desirable, is not called for just now, inasmuch as we recently, in October last, devoted an article to modern teaching in respect to the diseases of infancy and childhood.

We shall, therefore, on this occasion, content ourselves with simply notifying to our readers the appearance of the new editions, without attempting any comparative estimate of their value, and reserve to another opportunity a critical examination of the existing state of opinion and practice in relation to children's diseases. Dr. T. H. Tanner's 'Practical Treatise on the Diseases of Infancy and Childhood' has reached a second edition, revised and enlarged by Dr. Alfred Meadows. It is a very complete treatise of 472 pages, commendable for its typographical excellence ; and as to the character of its matter, Dr. Tanner's name offers a guarantee, for few writers of medical books have so successfully met the requirements of medical practitioners as Dr. Tanner did in the several publications on medicine he brought out. We have to regret his career of usefulness has been prematurely cut short.

The 'Practical Treatise on the Diseases of Children,' by Dr. Fleetwood Churchill, has, in the third edition now issued, grown into a thick volume of 900 pages, under the joint revision of the author and his son. It is a work that has largely recommended itself to the busy practitioner, as the production of a physician specially qualified to give instruction on the subject. A third work, on the diseases in general of children, is by two American physicians, Dr. J. Forsyth Meigs and Dr. W. Pepper, eminent as teachers in Philadelphia. The work first appeared as the production of Dr. Meigs, and passed through three editions. The one just issued is a fourth, and this fact sufficiently indicates the high opinion attached to the treatise by the profession in America. It exceeds in dimensions both the treatises before named, being a large octavo volume of 921 pages.

The next book is rather a treatise on preventive medicine than on the symptoms and treatment of disease. It is the well-known and highly valued work of Dr. Andrew Combe, on 'The Management of Infancy, Physiological and Moral.' It has reached a tenth edition, and this last edition will claim the special attention and appreciation of English medical men, on account of its having been revised and edited by the universally respected Sir James Clark, who was called away from his labours by death very soon after he had penned the preface, in February, 1870. As the last monument of the indefatigable labours of that eminent physician in behalf of his profession the work would assuredly be highly esteemed ; but, apart

from that circumstance, it is recommended by the fact itself of Sir James Clark having, at his advanced age and many years of extensive experience, found it so meritorious a work as to induce him to undertake its revision. For his revision has not been a formal proceeding, but has resulted in much valuable additional matter, and some general improvements in the arrangement of its contents.

The last book to be now noticed in this list is that by Dr. Eustace Smith, 'On the Wasting Diseases of Infants and Children.' It is a second edition, revised and enlarged, and contains 309 pages. The first edition met with a very cordial reception by the profession. This second one is somewhat enlarged, chiefly by the addition of two chapters. One of these is occupied with instructions respecting the feeding of children, and presents a series of carefully arranged dietaries.

Ruspini's Manual of New Remedies.¹ Fifteen years have elapsed since the last edition of this valuable handbook, during which time not a few additions have been made to our list of medicinal substances, and many great improvements effected in pharmaceutical processes. It is extremely well calculated to enlarge the acquaintance we have with what has been achieved in Italy, an acquaintance which, from the imperfection of international communication, is certainly much more limited than one would desire.

Apart from the general interest that attaches to the name of Ruspini, the author is chiefly known to the British medical public as the introducer of mannite. This substance obtained honorable mention in the International Exhibition held at London in 1862. Most unfortunately, at that time, several other substances which the author had brought with him perished on the way hither.

In going through the work we especially remark the strong favour he expresses for the salts of soda as therapeutic agents, in preference to those of potassa; his advocacy of oxide of zinc over the trisnitate of bismuth in general uses; his account of the peroxide of nitrogen, and the suggestion of its use in asphyxia, corroborated by complete success in an instance of suffocation from carbonic acid. These give sure proof of the author's ingenuity and zeal in science. The names of Polli, Mantegazza, Schirardi, Scarenzio, and others, are associated with that of Ruspini in the work. By the Professor Angelo Scarenzio, of Pavia, the preparation of cod-liver oil with proto-ioduret of iron, now in general use, known in Italy as the *olio iodato Scarenzio*, was first introduced into practice. The name of Polli is mentioned with high respect. There is a fair account of the sulphites; but it appears that, even in Italy, during the last visitation of cholera, they were largely replaced in practice by the carbo-

¹ *Manuale eclettico di Rimedi nuovi.* Di G. RUSPINI. Settima edizione.

Eclectic Manual of New Remedies. By G. RUSPINI. Seventh edition. Bergamo, 8vo, pp. 778.

lates, and carbolic acid, became the fashionable antiseptic. *En passant*, we remark a phenate of quinine proposed and prepared by Romei, military pharmacist.

Among the many interesting subjects examined may be mentioned the article on curare, that on the hydrate of chloral, those on coca and other nerve tonics, introduced to European notice by Professor Mantegazza, and various bitters derived from the same source—South America, are brought under description. The dry, green, or gelatinous hydrate of chromium—*idrato di chromo verde*, as used by Dr. Gemma, of Padua, in doses of from three to ten grammes for diarrhœa, deserves attention. There is also an ample notice on silicilose or spiroilose acid, otherwise the essence of Ulmaria. Ruspini compares it to camphor in its therapeutic action; preferable to digitalis in diseases of the heart, where the cavities are large and the parietes thin, as well as in dropsical effusions from serous membranes. Both the last substances named appear to have been brought forward in France by Hammon.

In the articles on helicine, on the hyposulphites of Churchill, on the tincture of guaco, Ruspini would seem to aim at writing the necrology of these drugs, rather than their history; he condemns them as futile and worthless. Kamala and other substances are well dealt with; but, without further enumeration of its contents, the book will be found to recommend itself.

Macdowall on Treatment of Wounds.—The title of this pamphlet is attractive. We took it up with eager anticipations, but we must confess that we have laid it aside with a feeling of disappointment.

The treatment of wounds is a subject of so much interest just now, that we looked anxiously to see what this “new method” was, which seemed to have been tested during the siege of Paris. But ‘Gruby’s System’ merely consists in dressing wounds with cotton-wool dipped in oil—what particular oil we are not told!—and the information which the author gives us about it is of the most meagre kind. It may be an excellent dressing—indeed, we have no doubt that it yields good results—but Mr. Macdowall’s account of it will not do much to recommend it to the profession. The rest of the pamphlet is occupied with some fragmentary observations upon the ambulances which were in use during the recent war in France, and with some very crude statistics of the operations which were performed, and of which a considerable number terminated fatally by pyæmia. Here is a specimen of our author’s note-book; and the same want of definite and precise information which it shows pervades the whole *brochure*. In speaking of the American ambulance, he says—

¹ *On a New Method of Treating Wounds (Gruby’s System), and the Medical and Surgical Aspects of the Siege of Paris; Outlines for a Non-official Report to the Physician to the Right Hon. the Minister of State for India.* By CAMERON J. F. STUART MACDOWALL, Surgeon, Indian Army, 3rd Bombay Light Cavalry, (present during the whole of the siege). London, 1871, pp. 35.

"One hundred and fifty beds; 260 admissions. No pyæmia (?); only one case of erysipelas; no gangrene; no scurvy (?); thirteen deaths in all up to *now* (February). Up to November only two deaths out of sixty-two admissions, one of them from tetanus. Dressed with finely picked and carded oakum and nitric acid lotion. The deaths were, four amputations of thigh (out of seven operated on), one with symptoms of tetanus, two of exhaustion (?), one of gastritis (?). No purulent deposits in viscera (?) (post-mortems were *not* made). Two cases of resection of elbow died (*out* of five operated), both secondary operations. Two amputations of leg from exhaustion (?), one with *rigors*. One compound fracture of ankle-joint, died; he had lately had smallpox. One of bullet through the liver died. One ditto through abdomen. One case of ball through lungs, grazing liver, died; and one ditto through lung—all from exhaustion(?)" (p. 14).

In these days, when we want a careful discrimination of cases, and accurate information respecting them, the value of such statements as these is simply *nil*.

The first ten pages of Mr. Macdowall's pamphlet are devoted to an energetic protest against the introduction into India of "calf-vaccination," a practice which the writer says has entirely failed in France, and which, in his opinion, would be attended with the most disastrous consequences in our eastern empire.

Mr. Macdowall deals with too many subjects, all of them interesting and important, within the limits of a pamphlet. Had he taken up one of them, and followed it out fully, his pamphlet would have been of more value.

Crookes' Quantitative Analysis.¹—This volume is not an ordinary manual of quantitative analysis. It contains full descriptions of new or improved processes for estimating the constituents of interesting or important minerals and artificial products, but it does not pretend to offer a complete system of analysis. It forms a supplement of great value to such treatises as those of Fresenius and Bunsen. Books of the same kind are in use on the Continent, but those hitherto published in England, with the exception of Dr. Hofmann's translation of Wöhler's small hand-book, now rather out of date, are not important. In the wideness of the subjects embraced, in careful editing, and in the particular attention paid to the rarer elements, Mr. Crookes' volume presents singular merits. Those analytical chemists and teachers who know what trouble they constantly experience in hunting up, from the depths of some chemical periodical, the details of a special process which seems likely to answer the end they have in view, will appreciate this volume of 'Select Methods.' Here methods are given in sufficient fulness, not by units, but by

¹ *Select Methods in Quantitative Analysis*. By W. CROOKES, F.R.S. London, 1871, pp. xvi and 468.

scores, or rather by hundreds; we can carry the volume into the laboratory and work from it, instead of spoiling the scattered but numerous books or parts of books in which the desired analytical directions originally appeared. But besides this advantage, this collection of analytical processes gives us a further benefit, as the author has personally tried many of the methods, and in some cases improved upon them. His well-known skill and accuracy have been brought to bear both as to the criticism and origination of analytical processes.

An adequate notice of Mr. Crookes' work can hardly be here given, yet we may advantageously particularise a few of the subjects which will be found to be handled with peculiar skill in the pages before us. They are such as the following:—The separation and estimation of such rare metals as cerium, rubidium, cesium, thallium, and indium; the recognition and estimation of ammonium compounds, of nitrates and of nitrites; the analysis of iron ores and of steel and iron; the detection and estimation of arsenic, antimony, sulphur, and phosphorus; the assay of animal charcoal, and the examination of its deodorising and absorbent properties; the valuation of coal-gas; the eudiometrical analysis of air and other gases; a collection of various new and approved plans for estimating phosphoric acid; and, lastly, a new method for the determination of the several elements of organic substances. A series of useful tables for the conversion of thermometric degrees, of hydrometer scales, and of English and French measures and weights, concludes this compact and useful volume.

The Medical Works of Francisco Lopez de Villalobos.—The writer whose works have been the subject of much study to Mr. Gaskoin, the first fruits of which are here given to the public, was a Spanish physician, and lived at the end of the 15th and the beginning of the 16th centuries. He was a person of considerable note in his day, both as a physician and a writer, and was attached to the court of Ferdinand, King of Spain, and after his death to that of his son, the Emperor Charles the Fifth. He is mentioned either very briefly, or not at all, in such biographical works as are within our reach, nor has Mr. Gaskoin been able to discover much interesting matter relating to him. Three of his works are contained in this volume;—a poem “on Syphilis,” a dialogue “on Tertian Fever,” and another “on Natural Heat.” The poem is written in Spanish, and the writer is commended by eminent critics for his purity and propriety in the use of the Castilian tongue (pp. 72, 73.) We confess we should have preferred that Mr. Gaskoin should have

¹ *The Medical Works of Francisco Lopez de Villalobos, the celebrated Court Physician of Spain.* Now first Translated, with Commentary and Biography, by GEORGE GASKOIN, K. Commander of the R. Military Order J. Christo of Portugal, &c. &c. Small 8vo, pp. 312. London.

given us a prose translation of this work, which would certainly have been much pleasanter reading, while it would probably have given the reader a better idea of his author's meaning than the singularly unrhythmical stanzas into which he has forced the poem. He says (p. 92), that he has endeavoured "to follow the metre as well as the sense of the original with the greatest possible exactness;" but he also (p. 3) confesses that he is "a total stranger to versifying," so that we must be allowed (for the credit of Villalobos) to doubt whether the translation gives a fair impression of the original. The work itself is interesting to the medical historian and antiquarian as being one of the earliest on the subject to which it relates, written probably in 1494, at the time of the supposed importation of the disease into Europe from America. We cannot enter here into the discussion as to the first appearance of syphilis in Europe, but we can refer our readers for much information on the supposed American origin of the disease to this book of Mr. Gaskoin (p. 220, &c.), and also to his interesting papers in the 'Medical Times and Gazette' (vol. ii, for 1867). At the same time it is only right to add that one of the most competent authorities on the subject, M. Daremberg, in his recent 'Histoire des Sciences Médicales' (p. 353), considers Mr. Gaskoin's arguments to be more specious than solid, and still maintains that syphilis was known in Europe some years before the time of Columbus. The title of the poem is "On the contagious and accursed Bubas," and Mr. Gaskoin in his introduction has an etymological section on the meaning of the word, in which he states (p. 81), that, "as used by the Spaniards, it has nothing to do with the Greek [*βούβων*] at all," but that it is "French or Provençal in its extraction, signifying nothing more or less than *eruption, breaking out or exanthema, push or pock*." Mr. Gaskoin's introduction, though quite unnecessarily diffuse, is interesting, and shows learning and acquaintance with the subject on which he is writing; and the same remark applies also to his Notes, in which, however, we are not quite sure that he is always trustworthy. For instance, he gives various extracts from old editions of Latin writers, but the mistakes made in converting the abbreviated words into the ordinary spelling are more numerous than we should have expected. It would almost seem as if he supposed Avicenna himself to be the author of the "Arabicorum nominum antique expositio," found in most editions of his works (see pp. 190-195.) He need not have appeared to express a doubt (p. 190), whether the *كَلِمِيَاء* *klimia* or *اِكَلِمِيَاء* *iklimia* of the Arabians was the *καδμεία* or *καδμία* of the Greeks, as two passages of Galen and Dioscorides relating to *cadmia* are quoted by Ibn Baitár under *climia* (vol. ii, p. 311, in Southeimer's translation). We should have been glad also if Mr. Gaskoin had given more

references in support of some of his statements and quotations, and occasionally we have a right to complain that the references which he does give are so vague as to be practically useless; for instance, we doubt whether "*Galen, primo reg. acut. comment.*" (p. 186), or "*Fen. xxii of Avicenna*" (p. 209) would be of much use to a person who wished to find the passages referred to. If Mr. Gaskoin will try to profit by these hints, we think he will be able to improve his forthcoming translation of some of the other works of Villalobos without much additional trouble to himself, and with a great increase of comfort to his readers; and we shall be glad to continue our acquaintance with the writings of his favorite author, even though we may not be inclined to rank him quite so high as Mr. Gaskoin himself.

Typhoid Fever in Islington.—Shortly before we met with the first account of Dr. Ballard's paper in the weekly medical journals we had been looking at Sir George Baker's account of his discovery of the true cause of the Devonshire colic, and we think we cannot pay a higher compliment to Dr. Ballard than by saying that his mode of treating his subject at once brought Sir George Baker's masterly essays to our mind. This favorable opinion of his paper was confirmed when we had an opportunity of perusing it in a complete form; and we are inclined to rank it among the most successful specimens of an inquiry conducted by the process of exclusion (the only process, as Dr. Ballard rightly says (p. 25), applicable to such an investigation) as near to actual demonstration as the case admits of. We cannot, of course, undertake to give here anything like a full account of Dr. Ballard's inquiry; we can only mention that when it became his duty as medical officer of health to investigate the circumstances attending a severe localised outbreak of typhoid fever in Islington, the suggested causes of the epidemic resolved themselves distinctly into four. Each of these he discusses at length, and dismissing the three most plausible as being quite insufficient to account for all the circumstances of the case, he at last comes to the singular conclusion that the outbreak could have been mainly caused by nothing else but adulterated *milk*, though it was probably aggravated, in particular cases, by local sources of miasm in some of the dwellings attacked. After he has satisfied himself (and his readers also) on this point, he proceeds to inquire how the typhoid contagium got into the milk; and this inquiry he conducts in the same way as the preceding, discussing and dismissing, one

¹ *On a localized Outbreak of Typhoid Fever in Islington, during the months of July and August, 1870, traced to the use of Impure Milk.* By EDWARD BALLARD, M.D. (Lond.), Fellow of University College, and Medical Officer of Health for Islington. (Being a paper read before the Association of Medical Officers of Health, and printed for the Association.) London, 1871. 8vo, pp. 35.

after another, several possible sources of the calamity, and finally proving, almost to demonstration, that the milk was contaminated by water taken from an underground tank in communication with two old drains. But this bare enumeration of the principal facts of the case gives, of course, no idea of the interest which Dr. Ballard has contrived to impart to his narrative, and which goes on increasing till at last it amounts almost to excitement. How the contaminated water got mixed with the milk we can only conjecture, for the young man who had the entire management of the business, and who, therefore, could probably have explained the whole matter, was himself one of the victims of the disease; but if we are unwilling to suppose that this water was purposely added by any one to the milk, we have the admitted fact that it was used for washing the cans (p. 29).

We cannot end this brief notice of Dr. Ballard's remarkable paper without expressing our satisfaction at his recent appointment in the Medical Department of the Privy Council. If this is the way in which Dr. Ballard conducts all his sanitary investigations, a fitter person for the post of Medical Inspector could hardly have been found.

Physiological Effects of Severe and Protracted Muscular Exercise.¹—About a year and a half ago Dr. Flint had the opportunity of examining the urine of Mr. Weston, a gentleman who had performed the extraordinary feat of walking 100 miles in twenty-one hours and thirty-nine minutes, and the results he obtained were at variance with the opinions that have been gradually gaining ground during the last few years. He found in fact that, as compared with the urine passed under ordinary conditions of health and exercise, the amount of urea contained in the urine after this protracted exertion was considerably in excess, which, supposing the diet to have not been materially different from that which was usually consumed, would indicate increased waste of the nitrogenous tissues of the body. But on this occasion no reliable data could be obtained in regard to this important point, and consequently no conclusions could be drawn. Quite recently, however, Mr. Weston undertook for a wager to make an attempt to walk 400 miles in five consecutive days, and upon one of those days to walk 112 miles in twenty-four consecutive hours. He at the same time offered to submit himself to scientific observation, not only during the five days of the walk, but for five days previously, and the same period subsequently. Dr. Dalton, of course, gladly seized the opportunity,

¹ *On the Physiological Effects of Severe and Protracted Muscular Exercise; with special reference to its Influence upon the Excretion of Nitrogen.* By AUSTIN FLINT, jun., M.D., Professor of Physiology in the Bellevue Hospital, Medical College, New York. 1871. Pamphlet, pp. 90.

and was fortunate enough also to obtain the skilled assistance of Dr. Doremus, father and son, and their assistant Mr. Oscar Loew, in weighing the food and in making the requisite analyses, whilst a committee of physicians was formed to superintend and offer suggestions upon the whole experiment. No attempt was made to interfere with or modify Mr. Weston's diet. What he was about to eat was weighed, and what was left was weighed, the difference being estimated as consumed.

The composition of the food was estimated upon the analyses of Payru, or where these were not serviceable special analyses were made by M. Loew. The fæces and urine were carefully weighed and analysed, and the weight and temperature of the body taken at stated intervals. The conditions of the experiment were therefore extremely satisfactory. Mr. Weston underwent but little training in the five days previous to the walk; he ate well, and the chief point of interest of this period is, perhaps, the great excess of urea he habitually discharged, his daily average being no less than 628 grains, or 33 per cent. more than the average given by Parkes and others (his weight being only 120 lbs., and his estimated excretion of urea being at 3.5 per 1 lb., only about 420 grains). The quantity of nitrogenous food injected, however, was very large. Mr. Weston failed to win his wager, but he walked 80 miles on the first day, 48 miles on the second, 92 miles on the third, 57 miles on the fourth, and 40½ miles on the fifth and last day, or 317½ miles during the five days. The elimination of urea during the five days of the walk was 722 grains per diem on the average. During the five days subsequent to the walk he took but little exercise, though he still continued to excrete an excess of urea amounting to a daily average of 727 grains. During the five days of the walk, Dr. Flint remarks, Mr. Weston consumed in all (omitting decimals) 1174 grains of N in his food. During the same period he eliminated 1808 grains of N in the urine and fæces. This leaves 634 grains of N over and above the N of the food which must be attributed to the waste of the tissues, and probably almost exclusively to the waste of his muscular tissue. Dr. Flint estimates the percentage of N at 3 per cent. in muscular tissue, and the 634 grains would then represent a loss of 21,127 grains, or 3.018 lbs. of muscular tissue. The actual loss was 3.450 lbs. This allows about 0.43 lbs. loss unaccounted for, which might be fat or water. We may just observe, in passing, that, according to the recent analysis of M. Petersen, the percentage of nitrogen in fresh meat is on the average 3.35, which would correspondingly modify the above figures, the 634 grains only representing a loss of 18,835 grains, or about 2.69 lbs. of muscular tissues, leaving .760 lb. loss unaccounted for. During the period of five days of rest succeeding to the walk, Mr. Weston gained in weight 4.5 lb., and retained in his system an amount of nitrogen equivalent to

1.1 lb. of muscle. As a general result of the whole inquiry Dr. Flint draws the following conclusion :—"If these facts be accepted, and, leaving the widest margin for error in the estimate, they cannot involve any considerable error, it is impossible to come to any other conclusion than that excessive and prolonged muscular exertion increases enormously the excretion of nitrogen, and that the excess of nitrogen discharged is due to an increased disassimilation of the muscular substance; and it is to be remembered that the experiments upon which this statement is based were made with a diet regulated solely by the taste of the individual." From the difficulty of attaining accuracy in such complicated inquiries as these we are not prepared to accept this conclusion as indisputable, though it certainly appears to us to be fairly borne out by the tables. We can confidently recommend the paper to the attention of those of our readers who are interested in this subject, as one of the most valuable contributions to our knowledge of this department of physiology that has yet appeared, and certainly gives a severe blow to the doctrine of late years, that the nitrogen of the urine is essentially derived, not from the disintegration of the muscular tissue, but from the assimilated nitrogenous compounds contained in the blood.

Dr. Dobell's Reports on the Progress of Scientific Medicine.¹—The appearance of this second volume of reports does great credit to the enterprise of Dr. Dobell. To endeavour to represent the progress of practical and scientific medicine in different parts of the world is a large and bold undertaking for any man, whether regarded from a literary or a financial point of view. Indeed, the very boldness and enterprise that mark the conception of such an undertaking challenge particular inquiry into the manner in which the task has been performed, and bring the producer within the range of a particular criticism. And the more so, inasmuch as he places his production into competition with already accepted works of a similar scope and character.

Following our usual course of honest criticism, we had, in our number for October, 1870, when reviewing the first volume, to make some sharp animadversions both on the professions and assumptions of the prospectus of the new undertaking, and likewise on the manner in which the former had been fulfilled. We have no wish, on the present occasion, to reopen a critique on the same point. Judging from the preface and the contents of this second volume, our strictures have not been lost on Dr. Dobell. He launched his project with too loud a bray of trumpets and with too many pretensions to universality and completeness, and he has consequently

¹ *Dr. Dobell's Reports on the Progress of Practical and Scientific Medicine in different parts of the World, contributed by numerous and distinguished Coadjutors.* Vol. II. London, 1871. Pp. 606.

now to tell us that the announced object of the work—viz. “to bring together in the English language original and independent reports from all parts of the world, written by distinguished men resident in the countries which they represent,” and differing “widely in scope and intention from the half-yearly abstracts,” and other retrospects hitherto published—must be understood with certain deductions and allowances.

Those foolish reviewers who interpreted his announcement to mean that “the work,” as placed before them in the first volume, aimed at presenting (in the words used) “reports from all parts of the world,” are rebuked for their folly in expecting retrospects from elsewhere than “the principal centres of civilisation” annually, other regions necessarily coming in for notice only when some progress can be recorded of them.

So, again, other misjudging critics are now enlightened to interpret the advertised character of the reports as original and independent, and widely different from the half-yearly abstracts and retrospects well known to the profession, as implying “abstracts of matter already printed in books, papers, and journals,” with what sprinkling “of original” and independent writing may be procurable.

With these interpretation clauses this second volume must therefore be examined by its readers. But we cannot let pass one breach of promise in the announcement of the character of the work, and of which no notice is taken in the present preface, viz. the non-fulfilment in some instances of the condition that the reports should be “written by distinguished men resident in the countries which they represent.”

It may be, readers have not lost much by some of the ‘Reports on the Progress of Medicine in Foreign Lands’ being allotted to gentlemen resident in England; but the promise made implied that Dr. Dobell considered it a recommendation of his undertaking that such reports should come from physicians resident in their native countries, and presumably better acquainted with the literature, teaching, and practice there in vogue, than non-residents could be. So far, therefore, it is to be regretted that Dr. Dobell made an engagement without means at hand to fulfil it; and allowing for the excellency of the reports from countries by non-residents, there can be no question that those written by medical men from the countries in which they themselves practise will be read with greater interest.

From preceding remarks it may be gathered that the major part of the contents of this volume is made up of extracts and abstracts of the current medical literature of the time, just as in the case of the published abstracts, retrospects, and periodical reports with which medical men have for years been familiar. So far, therefore, Dr. Dobell’s plan differs in no noteworthy feature from that of

those other publications referred to, excepting so far as certain of his reports are written by foreign physicians. Where a departure is noticeable, it is in the case of some of the contained notices written by gentlemen in foreign lands detailing their own experience and observations respecting climate, prevalent diseases, and special modes of practice.

In such a collection of papers, produced under the appellation of reports, and consisting mainly of abstracts from books and periodical literature; from countries of advanced civilisation, knowledge, and importance, and from newly settled lands, from semi-civilised communities, and from places of no importance in the great family groups of mankind, and written by men of repute in medical science, and by others whose claim to position has yet to be established, there is necessarily much variety in the composition, with regard both to the inherent quality of the matter conveyed and to the mode in which it is conveyed.

Moreover, the editor has suffered from many mischances, which have deprived him of a fitting account of the state of medicine in countries of importance (*e. g.* America); and in other instances he has evidently not as yet discovered the right man for the work; the so-called reports of the progress of medicine in those instances being of the most partial and superficial character, representing nothing in particular.

It will be a more agreeable task to signal out those reports which commend themselves as such, than to enumerate those defective productions just referred to. We are therefore pleased to mention the report from France by Professor Villemin, of the Val de Grâce; that from India by Mr. C. Maenamara, and that from Turkey by Dr. Sarell. The notice of German medicine, though not written by a physician resident in Germany, according to the strict letter of Dr. Dobell's advertisement, comes from the very competent pen of Dr. Althaus, who is well versed and interested in the literature of the fatherland.

Readers of the first volume will also be pleased to meet again with a report from Dr. Hjaltalin, containing notes of his Icelandic experience; and they may gather some notions of the special medical features of other little known places, such as New Zealand, California, Java, and the Shetland Isles, from the memoranda of practitioners living in those several localities, but to which the title of reports is not justly appropriate.

"The United Kingdom of Great Britain and Ireland" comes in—and as citizens thereof we should say rightly enough—for the lion's share of space. Out of the whole number of 570 pages, the collection of abstracts and extracts shadowing forth the sayings and doings of British medical men takes up as many as 224. To those who await the annual issue of Dr. Dobell's volume to learn what is going on about them in the professional world, this devotion

of space to notes and jottings from the more recent productions of the medical press, and especially from the periodicals of the day, may be satisfactory enough; but those, including certainly the majority of practitioners, who endeavour to keep themselves *au courant* with the progress of their profession, will, in all probability, view this preponderating section of the volume with less favour, seeing that it is to them a collection of facts and opinions already served up to them once and again in various weekly, monthly, and quarterly periodicals.

Before concluding this notice of the contents of the volume before us we must mention that Mr. Heather Bigg has continued in it the memoranda of mechanical appliances and instruments which in the previous volume constituted a leading feature, and one of considerable value, particularly in reference to the future history of medical and surgical appliances.

The best criterion of the need and value of such a volume of reports must be found in the amount of public patronage, or the success attending the adventure. Although we may not recognise the requirement for such a work, nor estimate the character, mode, and quality which the already published volumes have presented so highly as Dr. Dobell, yet as an instrument for good in extending medical science we cannot do otherwise than wish the undertaking success.

Descriptive Catalogue of the Warren Anatomical Museum (Harvard University).¹—For the copy of this very complete and admirably prepared catalogue, we are indebted to the Medical Faculty of Harvard University, by the hands of their Dean, Dr. Ellis. An 'Introduction' makes us acquainted with the formation and subsequent history of the museum. From it we learn that it owes its origin to Dr. John C. Warren, who occupied the chair of anatomy and surgery from 1815 until 1847, and was an assiduous student and a diligent collector of pathological and other preparations. "On resigning his professorship in 1847, the greater part of his collection was presented to the College, and with it the sum of 6000 dollars, for its preservation and increase. In acknowledgment of so valuable a donation, the corporation of the university voted that the museum should be called by the name of the founder."

The collection so auspiciously commenced has year by year been largely added to, chiefly by gifts from the university professors; but also, in some measure, by purchases made out of the endowment fund.

"The most valuable specimen that has ever been added to the

¹ *A Descriptive Catalogue of the Warren Anatomical Museum (Harvard University).* By J. B. S. JACKSON, M.D., Curator of the Museum, &c. Boston, 1870. Pp. 759.

museum, and probably ever will be, was given two years ago by Dr. John M. Harlow, of Woburn. It was the skull of a man through whose head a large iron bar passed, and who essentially recovered from the accident." It is a case that "seems generally, to those who have not seen the skull, too much for human belief." The particulars, therefore, are with great propriety given at large, and are further illustrated by photographs of the skull, showing the course the bar took and the extent of injury it inflicted. The man lived twelve and a half years after the accident, and ultimately died the day after an attack of convulsions, preceded by no marked symptoms of new or active mischief in the brain. The museum also contains the cast of the head of a man, "transfixed through the head by an iron gaspipe" (over four feet in length), who survived his injury with some mental weakening, and damage to sight.

Comments upon the specimens catalogued are out of the question, and it suffices to add that "the whole number of specimens now (1870) in the museum amounts to 3680." The medical school attached to Harvard University may well pride itself in the possession of so valuable and extensive a collection of objects calculated to instruct its pupils both in normal and in pathological anatomy.

Original Communications.

I.—On the proper Management of Tedious Labours. By Dr. G. HAMILTON, Falkirk.

IN the year 1853 I inserted a paper on this subject in the pages of this Journal, in which I propounded views which were then somewhat novel, and stated results arrived at in my practice in cases which had been strictly under my own care, which I have no doubt were considered by most practitioners as rather startling, and especially so by those who had studied the subject mostly in hospitals. But if these results were then considered startling, my subsequent experience, as I have given it in different numbers of the 'Ed. Med. Journal,' and in my "Reclamation," inserted in this Journal in January, 1871, is still more so, and I frankly confess has astonished myself as much as it may have done others. My first statement in this Journal was, that I had brought into the world successively 317 children, all of whom had been born alive, with the exception of one, in a breech presentation, and that somewhat more than one eighth of these had been delivered with the forceps; next, that under the same treatment the numbers had increased to 1 in 467, or 416 successively; and, finally, that 731¹ had been born alive successively, the 732nd child having been stillborn. That case occurred in December, 1860, and I now in September, 1871, have to record that from then up to the present time, and under the same management, in all my own practice I have lost only one other child, in a footling case, and in all the forceps cases not a single child. *In other words, that every head presentation in both series has yielded a living child.* Even the 732nd case, where the head presented, would in all probability have done the same had I not been compelled, from convulsions coming on in the mother, to interfere more than I would otherwise have done with the delivery of the child.² What mortality, therefore, I have had among the children has fallen almost entirely on the breech and footling cases. The two referred to were of this description; and another very interesting case of the same kind, of which also I have given the particulars in the 'Ed. Med. Journal' for May, 1855, was only just excluded from the "stillborn" list.

This, as I have elsewhere said, is such an extraordinary departure

¹ See 'Edinburgh Med. Journ.,' May, 1853, and October, 1861.

² See an account of this case in 'Edin. Med. Journ.' for Oct., 1861.

from anything that I am aware of in the history of obstetrics, and presents such a serious view of the possible sacrifice of human life that may have hitherto been going on in this department of our profession, that I am sure my professional brethren will be not unwilling to hear from me, after eighteen years of additional experience from first writing on the subject, even if I should have to repeat some things I have said before, how I think I am able to explain the very opposite results that have been attained by me compared with those of many other professional gentlemen for whom I entertain the very highest respect. And this is the more necessary because, although I know, both from friends privately and from what has met my eye in different journals and treatises, that my practice has been warmly approved of, and very ably supported, in some quarters, I am nevertheless aware that in others this has not been the case. And I still further must say that I cannot but lament to see that in two maternity hospitals of the two chief cities in Scotland (I have not lately seen the returns for any others), the infantile mortality should still agree so little with that of my own practice; for I find, on reference to the Annual Reports of these two institutions for the last three years, that the infantile mortality in the Edinburgh Hospital ranges from about 1 in 5 to 1 in 29, and in the Glasgow Institution from 1 in 11 to 1 in 27.¹ Now surely the discrepancy here shown, compared with the results I have given, must admit of some explanation, if not of amendment; and although I grant the subject is a delicate one, upon which I would speak with all kindness, yet I think both the interest of the public and the character of our profession require that it should be investigated. I shall say something more on this subject presently, and shall endeavour to assign some causes for the differences I have noted. In the meantime I would invite those of the profession who are interested in the question, to discuss it in an inquiring spirit, and in such a mode as that we may know exactly what we are talking about, and so as that we may free it as much as possible from errors that may be involved in general deductions.

For this purpose I propose, as the most simple method of inquiry, that we should limit ourselves, 1st, to the mortality to the children, because I believe that the maternal mortality ought always, *ceteris paribus*, to follow nearly the same ratio.² 2ndly. That we should

¹ Taking up the Reports of this latter Institution quite at random, as given in the 'Glasgow Med. Journ.,' to obtain numbers similar to my own unbroken series, I find the following:—For quarters June and Sept., 1869, and Feb., 1870, living children, 791; stillborn, 91; total, 882. Forceps, 17; version, 12.

² In the 731 cases referred to I have said, in the 'Edin. Med. Journ.' for Oct., 1861, "There were six maternal deaths, but in only three of these were the forceps used. Of the latter cases one died from disease of the heart, and another from asthma;" so that in reality there was only one case in which the forceps could have to do with the death. Since then, my practice as to maternal mortality has been entirely satisfactory; but, from the occasional occurrence of epidemics of

exclude all children that were either not viable, or were clearly dead when the labour commenced. 3rdly. That we should clearly distinguish cases which had been wholly from those which had been only partly under our care. Not to do so is to import a source of error into our calculations of the most grave description; for, by doing this, we not only would make ourselves responsible for, it may be, the bad practice of others, but would bring into our own numbers only their difficult cases. This plan has not been generally followed; but, until it is, I must express my opinion that little progress can be made in a scientific investigation of the question; and whatever objections may be held as applying generally, and in many instances justly, to statistics, I think the enumerations here are so simple, that we can hardly be deceived by them. The question which I wish settled is no more than this—given a number of children, in how many, when they were born, did the heart beat and the child breathe, and how often in these deliveries were the forceps used, and the result?

Looking back on my practice for forty years in this department, one of the most striking of its late characteristics has come to be, it appears to me, its simplicity. A mere enumeration of some of the negations that have occurred in it will show this well. For example, 1st. I never now use bleeding or antimony to relax rigidity of the os uteri. 2nd. I never now, except in special cases, use *secale cornutum* to hasten labour. Only a few times during the last thirty years have I required its assistance. 3rd. I now rarely, if ever, interfere with the first half¹ of labour. 4th. I now never require my patient to have supports for her feet, or a pillow between her knees. I simply ask her to lie on her left side, and keep her knees well drawn up; and I have only occasionally, even in applying the forceps, to ask her to shift her position to the front of the bed. 5th. For thirty years I have not I think once, in my own practice, had occasion to use the catheter. 6th. During this period I have not, in forceps cases, had occasion to ask the assistance of any of my professional brethren. This I mention, not certainly in a spirit of self-sufficiency, but for the purpose of showing how comparatively easy my practice has been. 7th. I never grease the forceps before introducing them, as I think this tends to make them slip. On the

puerperal fever, or from there being a prevailing tendency to the supervention of puerperal peritonitis, this never can be made the basis of safe statistics in such an inquiry as we are engaged in. In one of these epidemics a cluster of upwards of twenty maternal deaths occurred some years since in the hands of one practitioner in this district.

¹ In my former papers I stated that I divided labour into *two* parts, each of which I designated "half." My reason for doing so was that labour was then usually divided into "three stages," and I wished my "half" to be clearly distinguished from "stage," as the term was then employed. As the term stage is perhaps the better one, and as the division into three stages seems to me practically unimportant, while mine into two is really of great moment, I shall in future use the term stage as synonymous with half.

contrary, I strongly recommend that the inner surface of the blades be slightly smeared with India-rubber paste, which soon dries, and enables them to lay firmer hold of the scalp than does the bare metal. 8th. I never now use forceps with a double curve. All my cases, during the period alluded to, have been delivered with Ziegler's straight forceps. 9th. I never now, in my own cases, see the parts of the mother injured more than in an easy natural labour. As for rupture of the perinæum, it never happened to me in all my practice. 10th. Although I do not absolutely refuse the use of chloroform, I use it as seldom as possible, and in certain cases decline to give it as inadmissible. I generally require in tedious labours the full power of the uterus, and I have found that chloroform often deprives me of this. 11th. I never, if I am able, apply the first blade of the forceps otherwise than over an ear.¹ 12th. My forceps have no notches on the handles, for tying them, as I entirely disapprove of the practice.² 13th. And finally, it is pleasant for me to be able to say that, as a result, I almost never have stillborn children.

On several of these points I shall have to speak more fully as I proceed.

Having premised these negatives, it will be found, as I have said, that the management of the two stages of labour is thus considerably simplified. By the first stage I mean from the commencement of the pains till the full dilatation of the os uteri, and the entry of the head into the pelvis; by the second stage, from this till the completion of labour. As I have said, I now rarely attempt to interfere with the progress of the first stage of labour, even when this is protracted for some days. Indeed, when I can, I keep as much as possible out of the way of my patients, recommend them to walk about or lie down as they may incline, to take a little sherry and water to support the strength; and, in fact, I get over it the best way I can, without interference. As is well known, the late Professor Hamilton laid great stress on not allowing his first stage to last more than from twelve to fourteen hours,³ and I can recollect we, his students, were

¹ Dr. Sinclair, who writes in the 'Dublin Quarterly Journal,' in August, 1861, in support of the more frequent use of the forceps, says "they," the forceps, "are very seldom put on antero-posteriorly," and again, "we never required to feel the child's ear." Now by far the most general mode in which I apply them is antero-posteriorly, or nearly so, and I never, if possible, apply them without feeling an ear. Again, he says, "if we found the fetal heart about to fail after ergot, or otherwise, we used the forceps at once." But why then, if possible, use the ergot at all?

² Dr. Ramsbotham also says he disapproves of the practice; but still his forceps, like Smellie's, are represented with the notch; and the notch, as Dr. Barnes notices, is still preserved by instrument makers.

³ My much esteemed teacher used to enforce his views on this question by reference to the case of the late Princess Charlotte, who, he stated, lost her life from want of attention to this his favourite maxim. It is now well known that this most popular princess lost her life, not from want of attention to the professor's rule, but from flooding. Her child, however, no doubt lost its life from "tedious labour." Had the treatment of flooding and tedious labours been

particularly active in following out his directions, never going to a case without a supply of ergot in our pockets, and pestering our patients with our officious endeavours to hurry on the labour. All this I now believe to have been unnecessary and hurtful, and I imagine most of the profession are agreed with me.

Professor Hamilton and Dr. Burns approved of supporting and dilating the uterus, Dr. Ramsbotham did not; and Dr. Murphy, as far as I know, still holds the same opinion. I have in this matter always acted up to the instructions of my old teacher; and from the time I began to use the forceps more frequently than was usual, I saw that the importance of assisting the ascent of the uterus was increased, for until this obstacle has been removed the forceps can never be used with freedom and safety. It may therefore properly be said to be (although most necessary I think independently of this) a preparatory step to their application. I have, in thus dilating, encountered many rigid os uteri, but have rarely failed in accomplishing my object, by first giving plenty of time in the first stage for the parts to become properly prepared; and, in the second place, by using, when necessary, pretty determined force in the second stage. In doing this I have never met with a case where any unpleasant accident occurred to the uterus from tearing or otherwise, and the little extra pain inflicted on the patient I have usually found well borne, where she has been assured that this was necessary to help on the labour. Where the os uteri has been forced back towards the promontory of the sacrum, and has been kept there from the head pressing the uterus on the pubes, which latter in such a case may be found to project more or less inwards, or where, on the contrary, the head and promontory of the sacrum catch the uterus, or where, again, this is done, as sometimes happens, by both pubes and promontory, then I set myself to find out what is the exact cause of the detention, and introducing the hand more or less fully, determinedly push the uterus over the head. When this has been effected, I generally consider the rest of the case comparatively simple. I was called to a labour not long ago where a midwife had worked at the case for twelve hours without getting the uterus over the head. When I saw the patient the os was perfectly expanded, but the uterus was caught by the head on both the pubes and promontory of the sacrum, and I had to work hard for two hours and a half more before I succeeded. As time was precious after such long detention and hard work, I immediately applied the forceps and delivered, mother and child being perfectly well next day. Now let us for an instant reflect what might have been the consequences had the active measures used been neglected, and the labour had been protracted some eight or ten hours longer. The uterus thus caught

as well understood then as they are now, how wonderful might have been the differences at this time among reigning sovereigns.

must infallibly have become swollen and inflamed, and have blocked up the passage; and this in all probability becoming what has been called a "long forceps case," the instrument would have had to be applied under the most disadvantageous and difficult circumstances, the child very likely being lost, and the life of the mother endangered.

The more I investigate this point the stronger is my conviction that it is in many cases second in importance only to the use of the forceps. I am quite convinced, from my constant experience, that the excessive dread of many practitioners of injury to, or subsequent inflammation of, the uterus, in doing so with caution, and yet with firmness, is unfounded. At all events, I have not seen even a single case which has made me doubt its safety and propriety; and I can join with those practitioners in its condemnation only when it is used in the *first* stage of labour, which I have no doubt was formerly too much the case. I am inclined to believe that it is now becoming so clear as almost to be axiomatic, that while the safety of the child demands a more frequent application of the forceps than has been customary, the safety of the mother as decidedly demands that these should be applied as seldom as possible *within* the uterus. Convulsions, or some other exceptional occurrence, may imperatively require this; an ordinary labour, if it has been well managed, I should say hardly ever does.

It is very interesting and important for me to state, as having a connection with this subject, that I have never, in all my own practice, had a case of laceration of the neck of the uterus; and I believe the reason of this has been my constant and anxious endeavours to get the uterus over the head. In looking over the former numbers of this Journal, I find in 1851 a notice of Dr. Roberton's "Essays on Practical Midwifery," in which it is stated that he himself had met with ten cases of this kind, in which seven of the females died; and he has collected from different sources seventeen other similar cases. The reviewer says, "The sign of impending danger in these cases which Mr. Roberton thinks the most pronounced is a feeling of crampy pain and tenderness on pressure on some particular part of the lower abdomen; and he explains the cause of the crampy pain by referring to a case in which it occurred as the result of the cervix being held by a 'vice-like grip' between the head and the brim." The practice which Mr. Roberton counsels, if there is space for the head to pass, is "to watch the case attentively, to apply a binder, and, *perhaps*, to raise the caught lip of the uterus," &c. In such cases of catching of the uterus I have had no hesitation as to the practice to be pursued. I *must*, if possible, get the uterus over the head, and, as I have stated, I have very rarely indeed failed in doing so.

It is with the commencement of the second stage of labour, therefore, that our active interference should generally begin, if

we be called on to interfere at all. If the membranes have not been ruptured, and the head is presenting fairly, I then do so at once, and gently but firmly continue to press up the uterus with one or two fingers, or with the whole hand if necessary, until I have got it pushed over the head; and, as previously observed, I like to use the forceps as seldom as possible before this has been effected.¹ As this is especially necessary should ulterior help be needed, I never neglect it, and work assiduously at it till it has been accomplished, if possible. Sometimes I find myself unable to do so in a moderate time, and I then make up my mind to use the forceps, or other means for delivery, under comparatively disadvantageous circumstances. In my first paper in this Journal I gave it as my opinion that it is not safe, for the child especially, to allow this half of labour to continue much more than two hours, and I still adhere to that rule, as being of the very greatest importance. Much, more or less, as to interference, may depend upon the kind of labour we have to treat, but, as an average, I am convinced more than two hours cannot be trusted to; and in special cases, to secure a favourable result, we must shorten this time very materially.²

It is curious to note how Dr. Murphy tries to evade the necessity for interference with the forceps in relation to this point. He says, in this "controversy a new and very important question has been raised by Professor Simpson, which, if true, would decide in favour of interference in all such cases. He has shown from statistics that the mortality is increased in direct proportion to the length of the labour; that a labour of four hours' duration is more fatal than one of two hours, one of eight hours than one of four, and so on. Hence the inference that protracted labours are dangerous because of the *time* they occupy. We have given this important question the reflection it so justly merits, but confess we cannot coincide in the conclusions

¹ In one of the very few maternal deaths I have had, after using the forceps, I had been forced to use them within the uterus.

² In looking back at obstetric literature prior to this period, it seems strange now to see such an acute practitioner as Professor Hamilton stating that he had often supported the perineum for five or six hours ('Practical Observations,' 1840); Dr. Braithwaite, that he had attended 3000 labours, and had used the forceps in them only six times (Braithwaite's 'Retrospect,' 1843, p. 258); Dr. Murphy, that we must expect the mortality to the children (about one in five) to be much the same whether the forceps are used or not; and Dr. Collins, that in a given number of cases the forceps had been used twenty-four times, and the perforator seventy-four times. See, also, 'Brit. and For. Med. Rev.,' 1853, p. 516.

In a review of Drs. McClinton and Hardy's "Practical Observations in Midwifery," in 'Brit. and For. Med. Chir. Rev.' for 1848, the author says, "We will merely notice that out of 173 cases of tedious labour, delivered without instrumental assistance, thirty had ergot of rye, and only ten of the thirty children were born alive," although the vitality of the children had been ascertained before the drug was administered. And, again, "out of 259 cases of tedious labour, fifty-two were delivered by the perforator and crotchet, eighteen by the forceps, and sixteen by the vectis."

drawn from it. It seems to prove too much, that not only are the longest labours the most dangerous, but that the shortest are the safest; neither of these propositions has the support of our experience. The danger of protracted labour depends upon many causes; and if the constitution be good, *time alone* is the least injurious. Rapid labours are attended with risks from which those of moderate duration are free. We do not think therefore that the shortest labours are the safest, or the longest *in time* the most dangerous. The question must be determined by individual experiences."¹ No doubt Dr. Murphy is correct to a certain extent in this, and Sir James was clearly wrong in holding, from his statistics, that the risk to the child is in the ratio of the length of the labour *as a whole*, as I endeavoured to show in my paper in this Journal in 1853. But, granting this, will Dr. Murphy deny, or is there an experienced accoucheur existing who is not profoundly impressed with the conviction, that the ratio of mortality to both child and mother, but especially to the former, is most intimately connected with the duration of the *second half of labour*? And still further, after something like two hours, that the danger increases with every additional hour, not in a simple but in a very serious compound proportion?²

Holding these views, and the principle I have stated, as vital, I never delay in head presentations the application of the forceps, and I find not the slightest difficulty with my patients in doing so. As I have said, they or the attendants have never to be alarmed by extra assistance being required, and they are never alarmed by the "instruments" being sent for. If the case is at the least distance, I always have the forceps in my pocket; if near, I quietly slip out and provide myself with them, when I see they are likely to be needed. Generally, some time before applying them, I point out to patient and assistants that the pains are not doing good, or I tell them that the position of the head requires a slight rectification, and then, without the slightest fuss, I apply them and deliver. Sometimes this is effected immediately; in other cases longer time is required; and in the more difficult ones I take a good while before I succeed. The forceps will sometimes be applied and a few pains assisted; then they will be taken off and the patient encouraged to

¹ 'Dublin Quarterly Journal,' May, 1863.

² Dr. Ramsbotham, and other accoucheurs, as is well known, held that the child should be at or near the perinaeum from six to fifteen hours before the forceps should be applied. Dr. Murphy, following Dr. Collins, held, and I suppose holds still, that no interference is allowable as long as the head makes even the slowest progress, provided the mother does not show dangerous symptoms; and I think there was a danger about 1853 of this becoming the opinion and practice of some of our most eminent British accoucheurs. As far as I know I was the first to propound these two rules as to the two stages of labour, and I am glad to find that the latter, at any rate, has found general acceptance with the profession. See 'Brit. and For. Med. Rev.' for 1870, and my "Reclamation" in same, January, 1871.

take a few pains by herself; then they will be reapplied, perhaps several times, before I get complete power over the head. My plan always is to assist, and not to supersede nature. And here I may mention the superseding of nature as one of my objections to the use of chloroform. One of the chief difficulties I have encountered is in getting a good hold of the head, from its being too high up in the pelvis; in such cases smart pains are of great importance in bringing it within reach, the forceps being already introduced and ready to lay hold of it. But chloroform often dulls the pains, and in this way presents an obstacle to our operations; in general, therefore, I avoid it. For exactly the converse reason, it is in these cases, and in these alone, that I have used the ergot, except at the close of labour to prevent flooding. In using it to effect the purpose I have in view we run a certain risk of killing the child, but when the delivery can afterwards be effected quickly, it is, perhaps, good practice to do so. As I have already said, however, I have required to run this risk very rarely indeed. The danger of using it over and over again in the *first* stage, or indeed in either stage of labour, I think very few will now be inclined to dispute. As to the impolicy of usually indulging in the use of chloroform, I may give the following illustration: A lady whom I attended in a number of confinements had a tedious one, in which I gave her chloroform, and delivered her with the forceps without any difficulty. Subsequently she had another tedious labour, and she was so enamoured with the ease with which she had got through the previous one, that she insisted, positively insisted, that the same means should be again employed. It was of no use that I pointed out to her that this case was a little different from the former, as the pains here had died away, whereas formerly they had continued pretty strong. She still insisted, and became so impatient that I was obliged to yield. The consequence was, that although I easily got hold of the head, I had to do the whole work myself, and that what with me is usually a very simple matter, became a serious and protracted operation, in which the life of the child was endangered. This I told to the lady afterwards, at which, of course, she only laughed; but I protested most earnestly that I would never again be seduced into an error that gave me some uneasiness.¹

¹ I have two other reasons for avoiding the use of chloroform where *post-partum* flooding is likely to appear. The first is, that as the insensibility mostly continues some time after the delivery, the patient is unable to intimate to us, from her feelings, what is going on until serious danger may have occurred. I mentioned this many years since to Sir J. Simpson, and he at once admitted that under these circumstances we require to be very vigilant. My second objection to it is, that occasionally, after the delivery and when flooding is going on, it has the effect of exciting the patient, so as most materially to interfere with the use of measures necessary to restrain the flooding; and let no one who has not seen such a case think this a small matter, for I have found it one of the most appalling the accoucheur

And this last case has a bearing upon the *too frequent* application of the forceps; for it will be seen that, with all my admiration for this instrument, I grant there may be such a thing. In my own practice, as I have stated, the ratio has kept pretty nearly at rather more than one in eight, but I have seldom, unless in exceptional cases, kept in mind anything else than the safety of the child. When the mere temporary suffering of the mother is allowed to influence us, no doubt a more frequent application of the instrument may be required; but I am inclined to doubt whether the practitioner is justified in taking this very much into account; and whether the Frenchman's exhortation of "*Courage, Madame,*"¹ is not the best prescription we can give at these times. As a matter of course, when I say that I usually apply the forceps in relation to the life of the child, it is to simplify the matter, and because I believe that saving of the one implies also safety to the other. As the first Napoleon, however, most properly said, if there be the slightest inclination to doubt as to the safety of the mother, assuredly she should have the benefit of it. Still, my experience is, that an application of about once in eight deliveries is amply sufficient, in general practice, for both purposes. At any rate, also, I think I have shown that, in some instances, danger may attend an application of the forceps where we have not the assistance of labour pains; and these are generally the cases where we can afford to wait some time for their return.

The cases I have delivered with the forceps I would divide into four classes; the first being those where the head is well down in the pelvis, an ear easily felt, say near the right acetabulum, with the face to the same side. These, almost invariably, I have found it remarkably easy to manage. Gentle traction is applied, the head comes still further down, and the face passes into the hollow of the sacrum, sweeps along it, and the child is delivered. And yet, simple as they are, my belief is, that in hospital practice, among timid practitioners, and in the hands of midwives, it is in this class that the principal part of the mortality to the children will be found. The case probably is lingering, but "*everything is fair,*" the patient is not exhausted, &c.; valuable time is lost, and the child is stillborn. In these respects, while attending the Edinburgh Maternity Hospital, and in the early part of my own practice, I received some valuable lessons which I have never forgotten. Case after case occurred to me with dead children, where I see now it would have been the easiest thing in the world to have saved them. The very first out-

may have to encounter. Even without chloroform, when flooding is going on, the patient sometimes becomes very restless, or even almost unmanageable, but these I have found much aggravated where chloroform had been used.

¹ See an incident related in the '*Life of Dr. Combe*' on this subject; see also paper by Dr. Hardie, '*Ed. Med. Journ.*' for Dec., 1866.

door case I had at the Maternity was lost in this way. The woman was a primipara, and had lingered on for some two days without making much progress, till, on the second day, I asked the "annual pupil" to give me his advice and assistance. He, about as ignorant as myself, encouraged the woman to persevere—she would be better by and by. He was not able to use, or did not feel himself justified in applying the forceps; indeed, I am doubtful if they were ever spoken of, and the child, I now see, almost as a matter of course, was stillborn, which we, I am afraid, at that time, considered as a matter of not the slightest consequence. How far this system still obtains in Maternities I am unable to say positively, although, from inquiries I have made, there seems to be no great difference; but this I feel certain of, that wherever the attendant is incompetent or unwilling to deliver the child when in the position I have mentioned, and where any considerable time is lost in sending for assistance, death to the child in a large proportion of cases will be the consequence. In Maternities I would say that this above all things must be reformed, if it still exist, or the real seat of the evil will not be reached; and their mortalities will still sometimes, alas! in their annual reports inevitably run up to 1 in 5, or 1 in 11. Let well-trained competent practitioners reside constantly in the hospitals, whose duty it should be to be always at hand, and ready at once to give assistance in these simple cases, and let "consulting practice" be reserved for the rarer and more serious operations; and let out-door pupils be warned of this great danger, and have the means of getting assistance promptly. In this way practice in these institutions would be somewhat assimilated to that of private practitioners, and I should then be surprised indeed if the mortality were not very greatly lessened. I have had to impress these instructions with great warmth on the minds of midwives who have been in the habit of asking my assistance in difficult cases, and the result has been that, whereas I formerly lost about 1 in 7 of the children in these cases, I now lose a much smaller proportion.¹

¹ As testimony confirmatory of my own on this point, I may be allowed to quote an extract from a paper by Dr. Curran, of Dublin, from the 'Medical Press and Circular' of November, 1869:—"When studying midwifery," he says, "some years ago, at the Rotunda Lying-in Hospital, Dublin, I painfully noticed—and the observation has made no little impression upon me—that the students and embryonic midwives were compelled to allow poor women to continue in labour hour after hour until nearly exhausted, because the rules of the Institution forbade their interference, unless an over-fed and morose female superintendent was awaked and consulted. The educated student, revolting at such consultations, allowed the case to linger in preference. As I now reflect, I have not a particle of hesitation in saying that many of those confinements might have been safely and expeditiously concluded hours previously, had the best informed been allowed to prescribe a dose of ergot." For these cases Dr. Curran would have prescribed ergot, and I would not, but would, probably, in preference, have used the forceps; but still the bearing of the facts is the same. The above statement was, as mentioned, made by Dr. Curran in 1869, but I do not know to what period his

As an example of protracted first stage of labour, and of second cut short, from the really great importance attaching to the subject, I may give the following as among the latest that has occurred to me, though I confess that I feel the risk I run of being thought guilty of iteration:—Mrs. R—, æt. 20, primipara, had been in labour, under the care of a midwife, since the morning of April 9th. I was requested on the afternoon of the 10th to see her, and found the os uteri the size of a shilling, and the pains languid, but everything otherwise apparently going on favourably. I encouraged her to persevere, saying all was right as yet. On the morning of the 12th a messenger was again sent to me, saying that she was still undelivered, and that her strength was becoming exhausted. I found the woman weak, but there was nothing particularly wrong as yet. As the os uteri was now well dilated I ruptured the membranes, and in about an hour got the uterus over the head. I waited about another half hour, and then, as the pains were not sharp, and the advance of the head was slow, I applied the forceps, and delivered with the greatest ease in ten or twelve minutes. There was no caput succedaneum, nor was the head in the least misshapen; and yet, from what I have seen in other similar cases, I feel certain that a very moderate amount of further delay would most decidedly have put this child's life in jeopardy. I may remark, also, that this case shows the futility of Professor Hamilton's rule, and also of the inference drawn by Sir James Simpson from his statistics. Further, had the relative duration of the two stages been much different here, the result might have been very different also.

Once more, in concluding this part of the subject for the present, let me give the following extract from my note-book, dated May 2, 1871:

“Was called this morning at 8.30 a.m. to Mrs. B—, multipara. The membranes had ruptured last night, and a midwife had been with her since 3 a.m., who stated that the labour had been moderate,

experience at the Rotunda refers. I will say, however, and in this I think every dispassionate practitioner will agree with me, that if the same state of things now obtain there, or in any such institution in the kingdom, it may be said to amount almost to a public scandal. I beg to give it as my humble but very earnest opinion, that the directors and accoucheurs connected with these institutions may find here a fit subject for inquiry, if not reform.

In most of these institutions, as far as I can learn, the matron (mostly an opinionative midwife, a friend suggests) seems to be the person who exercises chief control, in the first instance. Then the “clerk” is sent for, and he possibly loses valuable time, and then at last the accoucheur is sent for. In the same way in out-door practice it seems to be, 1st, student; 2nd, out-door assistant; 3rd, accoucheur. Rule 4 of the Glasgow Maternity is, “Under no circumstances is a student to permit labour to be protracted for more than twenty-four hours without reporting the case to the out-door assistant in the first instance, and afterwards, if necessary, to the district accoucheur.”

With such a latitude as to time I should have great fears that my rule of two hours for the second half of labour will have very little attention paid to it.

but was now making little progress. I found the head well down towards the perinæum, an ear easily felt, and a small caput succedaneum. The patient had become uneasy and restless. I applied the forceps, and delivered, with the assistance of three pains, the cord being found round the neck, and short." On the above notes I make the remark, that I should have liked some of those practitioners who used, not long ago, to apply the forceps once in 500 or 600 times, to have been present and seen the ease with which this really trifling operation was gone through, and the safety and smiling comfort it at once brought into a household. They would, indeed, in such a case, in my opinion, have been "obstetrical reprobates" (the phrase is not mine, but Dr. Murphy's, which, he tells us,¹ he quotes from Dr. Blundell, *with approbation*, which I certainly do not) if they had failed to see and admit the vast benefits which the more frequent use of the forceps has conferred, and is likely in the future to confer, upon humanity. By no other operation that I know of, except, perhaps, vaccination, can such a saving of human life be effected, and in the great majority of instances I have found the performance of the one operation almost as simple as the other. If we take the annual births in the United Kingdom to be about 1,000,000, a lessening of the infantile mortality in these by only one per cent. would give us a saving of infant life in each decennial period of no less than 100,000.

(*To be concluded in our next.*)

NOTE.—The following are papers on this subject by Dr. H., relating both to mother and child, which have appeared in the journals:—"On Uterine Hæmorrhage," 'Ed. Med. Journ.,' Oct., 1850. "On the Mortality arising from the Use of the Forceps in Tedious Labours," 'Brit. and For. Med. Rev.,' April, 1853. "On Asphyxia Neonatorum and Infantile Mortality at Birth," 'Ed. Med. Journ.,' May, 1855. "Practical Observations and Suggestions in Obstetrics," 'Ed. Med. Journ.,' Oct., 1861. In "Proceedings of Edinburgh Obstetrical Society"—"On the Use of the Forceps in Tedious Labours;" "Uterine Hæmorrhage and Transfusion." "Reclamation," in 'Brit. and For. Med. Rev.' for Jan., 1871, in which the reader will please delete *s* in "dangers," at top of page 222.

Since the foregoing was placed in the hands of the printer I have been favoured, through the kindness of my old friend Dr. Longstaff, with the 114th 'Annual Report of the London Royal Maternity Charity,' dated February, 1871, and also with statistics of the deliveries at this Charity for the four preceding years. From these it appears that in the last five years the deliveries have been 17,265, and that the infantile mortality has ranged from about 1 in 30½ to nearly 1 in 35; though it is not stated whether this excludes children evidently dead previous to the commencement of the labours. This, it will be observed, is a considerably lower mortality than has been attained in the two Maternities referred to in Edinburgh and Glasgow; and it is just, I think, what might be expected from the different modes of management adopted. In the London Maternity neither students nor house-surgeons without proper powers are engaged; the deliveries being managed by a staff of qualified midwives, who can at once call in the assistance of appointed surgeons in cases of difficulty. There is thus much less loss of time in tedious labours where the midwives are employed—probably something like what occurs where a midwife in the country calls in the assistance of a surgeon—and, obviously as a consequence, a smaller loss of infantile life.

¹ See 'Dublin Quarterly Journal,' May, 1863.

II.—The Cholera in Copenhagen in 1866; the Precautions there taken against the Spread of the Disease; and the frequency of Diarrhœal Complaints in Denmark generally. By P. A. SCHLEISNER, Doctor of Medicine, Medical Officer of Health to the City of Copenhagen, &c. Translated from the Author's manuscript¹ by J. W. MOORE, M.D., M.Ch. Dub., L.K.Q.C.P.I.; Ex-Scholar, and Diplomate in State Medicine of Trinity College, Dublin.

ALTHOUGH in 1866 cholera prevailed as an epidemic in most of the Baltic seaports, namely, Prussian, Russian, Finnish, and Swedish, with which Copenhagen is very closely connected by the traffic of both sailing vessels and steamers, yet this city, notwithstanding that a considerable number of cholera cases were imported into the town through the navigation, was spared from any actual epidemic. To the city there were brought in all, besides 6 cholera corpses carried into the roadstead, 88 cases of cholérine, suspicious diarrhœa and cholera, of which 30 remained on board ship in the roads, while the remaining 58 were admitted to and treated in a cholera hospital fitted up in the city in the neighbourhood of the harbour, and which was kept isolated. The more accurate details concerning these facts will appear from Tables I and II.

TABLE I.—*Patients with Cholérine, suspicious Diarrhœa and Cholera, who remained on board ship in the roadstead of Copenhagen.*

1866.	Cholérine.	Choleraic-diarrhœa.	Cholera.	Cholera-typhoid.	Total.
May . .	1	1
June . .	2	1	3
July . .	3	6	3	...	12
August	4	4
September . .	2	4	6
October . .	1	2	3
November	1	1
Total in the roads = 30, of whom 2 died.					

¹ I take this opportunity of conveying to Dr. Schleisner my acknowledgment of the kindness and courtesy with which, in answer to an inquiry as to the prevalence of cholera in Denmark, put by me to Dr. Rasmussen, of Copenhagen, he was good enough to draw up the following valuable paper expressly for this 'Review.' At the present time, when cholera is again raging in some parts of Eastern and

TABLE II.—*Patients admitted to the Cholera Hospital, Copenhagen.*

1866.	Cholérine.	Choleraic-diarrhœa.	Cholera.	Cholera-typhoid.	Total.
June	1	...	1
July . . .	11	3	2	...	16
August . . .	5	10	3	1	19
September . . .	8	3	1	1	13
October . . .	5	2	5	...	12
Total, including 3 patients admitted from the city = 61, of whom 6 died.					

In the city itself only four cases occurred, of which the first was reported on the 8th of October from the cellar of a house chiefly inhabited by indigent folk. This patient died the following day, and on this very day from the same cellar two further cases were reported. These were immediately admitted to the hospital, where both shortly afterwards terminated fatally. About eight days subsequently the fourth case, which should, however, be regarded rather as one of severely marked cholérine, was reported from another part of the city. The patient became convalescent after treatment for a short time. Since this no case has shown itself in the City of Copenhagen. It is my conviction that the first instance of the disease which occurred in the city itself, had been produced by contagion from one of the patients brought in from sea. But, as it is on the whole extremely difficult in a large town, where the means of communication are so many and so complicated, to trace and follow up the propagation of a contagious disease, so in the present instance it was not possible to establish the fact of contagion, notwithstanding all the investigations set on foot by the Department of Public Health. It would only appear that the first of these cholera patients was married to a coal porter, who was frequently employed on board foreign ships, and who was also in the habit of bringing several of his comrades engaged in the same occupation down into the cellar in question; he himself had not had any symptom of cholera or of diarrhœa. A surmise, which was suggested, that his wife had bought clothes belonging to dead sailors—a form of traffic which, to my certain knowledge, has in several places introduced the cholera—is proved on further inquiry to be unfounded in fact.

Central Europe, the information respecting the precautionary measures so successfully carried out in the Danish capital, contained in the following pages, must be regarded as of the highest importance.—*Translator.*

That in 1866 Copenhagen, speaking generally, was in no slight degree exposed to a cholera invasion is evident from the fact that during the most dangerous period (from May 6th to November 10th), 2233 ships arrived at that city from *cholera-infected localities*, and sought communication with the shore, among which were 1490 bound for other ports, and so making only a temporary stay in the outer roads (for provisioning purposes, despatching telegrams, &c); 286 anchoring in the inner roads; and 457 passing within the bar in order to make a longer or shorter fixed stay in the harbour itself. The circumstance that Copenhagen was notwithstanding spared from cholera in 1866, and this too especially when most of the Baltic ports were attacked by this epidemic, must unquestionably, apart from the various hygienic improvements which within the last decades have been introduced there, in the most essential particular be ascribed to the measures adopted in that city. This supposition is strengthened by the fact that in Christiania, where somewhat similar measures were taken, a like result was obtained; while both Stockholm, and several other Swedish towns, which had not adopted measures of the kind, were severely attacked by cholera epidemics.¹

I would accordingly deem it appropriate to give a brief account of the measures here adopted, as they might possibly be imitated in other places, and I feel myself the more induced to do so as the Danish territory, owing to recent occurrences, is so very circumscribed, and its language moreover so little understood beyond the confines of the country. In the first place, however, I will offer some illustrative remarks in explanation of the organisation of our system of hygiene. In Copenhagen, where the medical central authority of the country has its seat, the Board of Health is composed of the following local sanitary authorities, namely, the Committee of Public Health for Copenhagen, with the Director of Police as President; the Committee of Quarantine (which has existed since the passing of the law of quarantine of 1805), with the Chief Inspector of Customs as President; the Medical Officer of Health to the City, who is at the same time a member of the two committees already mentioned; and lastly, the Quarantine Medical Officer, who has, however, only a semi-official position, as he is neither

¹ At Christiania, where, as here, removal of the inhabitants of attacked houses was resorted to, as also subsequent disinfection, &c., and where, as with us, the check to the contagious character of the disease was given by means of the measures adopted and carried out, the result was attained (according to a kind communication from the Committee of Public Health at Christiania to that here, and dated October 19th, 1866) that the whole epidemic was limited in number to thirty-six patients, of whom twenty-seven died.

appointed nor remunerated by the State or municipality, but is elected by the Committee of Quarantine, and is paid according to a fixed scale of salary. Quarantine itself, in the ordinary acceptance of the term, was in accordance with the law of the 10th of March, 1852,¹ abolished in Denmark in respect to cholera and yellow fever, since here, as in so many other places, we allowed ourselves to be led by the example set by England. Meanwhile experience soon showed that the repeal of every precaution of quarantine against cholera was a step in the wrong direction, and, after the make-shift substitution in the interim of the administrative measures of the Government, on May the 1st, 1868, was passed a new "Law respecting the Measures for preventing the Introduction of Asiatic Cholera by sea."² This law, however, has by no means again established quarantine, but has merely introduced *several inoppressive provisions* having reference to the navigation, essentially calculated to control the introduction of cholera patients, and to effect their speedy admission to special hospitals. A copy of both the laws in question is appended.

The measures, then, which were adopted here in 1866, and which I further intend to follow should the city again be exposed to a cholera invasion, consist in the following:

1. In conformity with the earlier provisions of the Department of Justice, all vessels, which, having sailed from a cholera-infected or suspected port, entered the roadstead of Copenhagen, were examined by the quarantine medical officer. The vessels, which either anchored in the inner roads, or lay-to in the harbour itself, underwent a direct medical examination, carried out on board. With respect to the so-named vessels of call (those bound for other ports), which made but a temporary stay in the outer roads, they were allowed to pass with a mediate examination or questioning of the disembarked crew—a rule of conduct that later became recognised and more accurately regulated by a decree of the Department of Justice, dated October 19th, 1870. The vessels which had patients sick or dead of cholera on board did not obtain permission to land passengers or crew until the sick had been brought ashore with the necessary measures of precaution, the cholera-corpses had been removed, and the ship had been properly cleaned and disinfected. This preliminary isolation of the vessel, to which the entire stay in quarantine was confined, was not extended beyond twenty-four hours. The method in which the examination of the vessels and their further management was conducted was devised in accordance with a fully detailed instruction issued by the Committee of Quarantine for quarantine medical officers on the 21st of June,

¹ *Vide* Appendix No. I.

² *Vide* Appendix No. II.

1866, in which, amongst other things, the passage occurs: "That every case of cholera or suspicious diarrhoea in persons arriving from a cholera-infected locality, as regards contagion, shall be looked upon in the same light as undoubted cases of cholera."

2. Further, the vessels arriving from the nearest Baltic (cholera-infected) ports, particularly those which lay in the inner harbour, besides being examined in the first instance by the quarantine medical officer, were also for the first three days of their stay in the harbour subjected to a medical inspection daily; while the harbour, with a view to this, had been divided into three districts, each under the supervision of its own medical officer.

3. All masters of lodging-houses, but especially such as might be supposed to receive seafarers on their arrival or journeymen mechanics as lodgers, got strict instructions not to house any patient suffering from cholera. This provision was enforced in accordance with the regulations for lodging-houses and inns previously issued by the Committee of Public Health. In order to examine the suspicious or doubtful cases reported by the masters of lodging-houses, a special physician was attached to each police-station.

4. In an isolated situation near the harbour, a hospital with twenty beds was prepared, intended to receive the cholera patients on the arrival of the ships, and the first sporadic cases that might eventually occur in the city. This cholera-hospital, as long as suspicious cases were admitted to it, and its officials, were kept entirely isolated from the rest of the city. A larger hospital with 100 beds was further kept in readiness to receive the cases which might subsequently occur in the city itself.

5. To all hotel-keepers and inn-keepers, and to railway directors, an appeal was made relative to the necessity of disinfecting the latrines, for which purpose the Committee of Public Health recommended suitable disinfecting remedies.

6. With respect to the care for the public health in the city itself: pursuant to the earlier adopted plan, there were instituted by the skilled police, with the co-operation of the members of the Public Health Committee, comprehensive investigations as to the trades injurious to health, and concerning houses which, as bad or overcrowded, it was considered should be subject to the special supervision of the sanitary police. By means of these measures such an organisation was effected that a constant and active control under guidance from the central police station, the director of police and his inspectors, might easily be exerted by the police assistants, each in his beat, over all the trades injurious to health, among which might be instanced, for example, the keeping of cow-stalls, slaughter-houses, piggeries,

&c. Many important restrictions and sanitary improvements were immediately effected in this direction.

Regarding the examination of bad and overcrowded dwelling-houses: for this, when extended to a very considerable number, a special minute-book was kept by the sanitary police; but unfortunately no important improvement or alteration in this direction was introduced, since the authority of the Public Health Committee, according to the existing laws, is very limited in respect to such.

7. All the institutions, orphan-houses, schools, &c., in connection with the Poor-Law system, were likewise examined, at the instance of the medical officer of health to the city, and a strict control was exercised as to the state of cleanliness of these institutions, so long as the threatening prospect of a cholera epidemic existed. The physicians to the respective institutions were further enjoined to make a weekly report concerning the possibly present disposition to diarrhœa and cholérine.

8. Lastly, as to the removal of the occupants of such houses as might first be attacked by cholera—a measure of precaution on which the Committee of Public Health must lay particularly great stress after the experiences which Professor Hornemann, who deserved so well of our entire sanitary system, had previously had an opportunity of acquiring—it may be mentioned that this rule of conduct was immediately enforced in connection with the first three cases which occurred in the city itself, and that it also succeeded here in its design, as many cases of cholera did not subsequently follow in the city.

With respect to this measure of precaution, the *removing* from, or *temporary evacuation* of, the attacked houses, I may, however, express myself more in detail. It is possible to carry out this rule in two ways. Taking, as a starting-point, Pettenkofer's theory, in accordance with which the cholera secretions are admitted only to possess the power of conveying infection *indirectly*; in other words, after having passed through a process of fermentation in the soil, of which, consequently, the entire importance depends on the nature of the soil, then the house must be completely evacuated, and all its occupants removed. This was the plan which had, on an earlier occasion, been carried out in this city, and that, too, on a tolerably extensive scale, in 1857, when we experienced a small epidemic, which, from August 15th to November 25th (the end of the outbreak), attacked a number of persons, amounting to sixty-nine in all, of whom fifty-five died. This measure of precaution is, however, unusually expensive, and in many towns could not possibly be carried out, by reason of the want of available dwellings. Fortunately it is not absolutely necessary. Setting out, on the

other hand, from the simple apprehension of the direct contagiousness of cholera, according to which the cholera secretions are considered the most essential bearers of the infection, we may confine ourselves merely to the evacuation of the separate apartments or closets in the house, while all persons are removed who have been in the room, or have had frequent intercourse with the patient (amongst others especially *attendants*, *watchers*, and *laundresses*). This is what I would call the *partial evacuation*, which we brought into use in 1866, and which proved capable even of cutting short the epidemic in its commencement, but was not successful in 1857. If this precautionary measure is to have any significance it should be carried out *speedily*, immediately after the announcement of the earliest cases; further, the respective apartments or closets in the house to be evacuated may justifiably be cleaned and disinfected, for which purpose it is likewise necessary that the removed occupants should be kept away for some time (for from seven to fourteen days). Again, an absolute condition for a successful result is that the sanitary police should receive timely *information* from the occupants concerning the first suspicious cases met with. This, however, is, unfortunately, seldom or never fulfilled.

I will, moreover, add that, for my part, I have very good reason indeed for laying stress on the adequacy of the above described measures, in accordance with the quarantine arrangements adapted to the requirements of the present time;—since from my former occupation, for ten years, as Medical Inspector of the Duchy of Schleswig, I had full opportunity of gaining experience in this direction. Among other things, the result was obtained in the great cholera year of 1853, that the disease in the Duchy of Schleswig was confined to some very few isolated and trifling outbreaks, while both Jutland and Holstein were tolerably severely attacked by cholera. Consequently I should be disposed to assume that, with the aid of these modified quarantine regulations, in combination with the system of partial removals above described, we should always be in a position, *at least in all towns of moderate size*, to check or cut short the outbreak of a cholera epidemic with almost as much ease as we can cut short, in the country, a commencing epidemic of smallpox or dysentery.

In conclusion, I will also add that we have likewise very lately adopted the system of partial removals in this city, although in a somewhat modified form, with the view of checking the spread of exanthematous typhus, and, at least as it has so far proved, with apparent success. In the last forty years we have known only one form of typhus in this country, namely, typhoid fever, with its characteristic intestinal lesions. Within the last two

years, however, several cases of exanthematous typhus have pretty frequently been brought to the city, chiefly from French, English, and Dutch vessels; and it would almost appear as if this form of typhus, which has also shown itself here as being extraordinarily infectious, and more than three times as fatal as our endemic typhoid, were about taking fast hold in Copenhagen.

Respecting the frequency of diarrhœal affections in our country, I will give below a tabular review of such; but first I will also make some remarks, the better to illustrate the basis on which our medical statistics are constructed. We have not amongst us any system corresponding to the English civil registration of births and deaths, since with us these are registered by the clergy. The statement of mortality, however, including the stillborn, amongst us should be more accurate and reliable than in most other countries, partly from the fact that in our country only a few religious sects are found dissenting from the popular church, and partly from a custom, more than a hundred years old, that no one is buried without the clergyman throwing on the earth, in consequence of which it is here extremely difficult to conceal a case of death. The registration of the causes of death, depending, as it does, on the death certificates drawn up by the physicians, has as yet been carried out only in Copenhagen and the market towns—not generally throughout the country. No doubt the Diet which has lately closed passed a new law relating to the inspection of the dead (that of 2nd of January, 1871); but this, with reference to the enlargement of the domain for the registration of the causes of death, has proved a great disappointment to the medical profession and to every friend of advantageous reforms. This is the more to be regretted, as Denmark was just suited for supplying reliable statistics of the causes of death, because (as in Sweden and Norway) we have only one class of scientifically-instructed physicians, who in Denmark and Norway respectively go out from one and the same university. The Statistical Department, which with us elaborates the various statistical materials, including the mortality and the causes of death, is, from economical considerations, supplied with very frugal means, and the fact that, notwithstanding this, the department has successfully elaborated and published a valuable and (proportionally to our means) a tolerably comprehensive mass of material, is chiefly owing to the highly accomplished chief of the department, Councillor David, who, from the different statistical meetings, has become well known, even in England, at least to England's celebrated statistician—William Farr. But though we are thus so much behind England with respect to the want of a system of civil registration,

&c., still we do possess an institution which is of great importance both for the care of the public health and for the medical statistics connected with it; and I believe England may well envy us our possession of the system. I allude to the staff of official physicians (district and town medical officers and physicians), outspread and distributed over the entire country. In addition to other matters, the duty has been imposed on these official physicians (in accordance with a decree of the Department of Justice, bearing date November 5th, 1861) of collecting and working up in a tabular form systematic lists of the cases of epidemic and other diseases commonly met with in practice. These it is incumbent on *each practising physician* at the close of the year to hand in, and the College of Health then conducts the final editing of the yearly lists furnished by these official physicians, and has them printed under the title of 'Annual Reports of the College of Health.'

The review of the frequency of the epidemic diseases in these annual reports is, however, two years in arrear, and at present only that for 1868 has come to hand. In Copenhagen the arrangement is that all the practising physicians (pursuant to the decree of the College of Health of the 17th of June, 1854) are obliged to send in weekly to the Medical Officer of Health to the City a list of all the cases of epidemic disease under treatment, according to an established plan. These lists are collected by that officer, who has them published week by week in one of the weekly medical journals. Having thus illustrated the arrangements connected with our medical statistics, I will present the appended tabular review of the fatal cases of diarrhœal affections both in Copenhagen, and of the cases of this kind treated by all the physicians throughout the country.

TABLE III.—*Deaths from Diarrhœal Affections in Copenhagen during the decennial period 1860—69.*

Diseases.	1860.	1861.	1862.	1863.	1864.	1865.	1866.	1867.	1868.	1869.	Yearly average.
Typhoid fever	56	58	55	78	246	122	88	88	104	53	94·8
Cholérine and diarrhœa .	59	41	39	41	79	152	85	100	157	69	82·2
Asiatic cholera	6	1	0·7
Endemic dysentery	3	2	4	3	4	4	5	3	2	3·0
Total .	115	102	96	123	328	278	183	194	264	124	—

TABLE IV.—*Deaths from Diarrhœal Affections in the Provincial Market Towns during the Quinquennial Periods 1860—64, and 1865—69.*

Diseases.	Average of 1860—64.	1865.	1866.	1867.	1868.	1869.	Average of 1865—69.
Typhoid fever .	174.4	240	202	105	232	122	180.2
Diarrhœa . .	43.2	115	48	67	168	43	88.2
Cholera ¹ . .	0.2	4	7	2	6	1	4.0
Endemic dysentery	1.4	4	...	3	4	2	2.6
Total	363	257	177	410	168	—

TABLE V.—*The Number of Patients under Treatment for Diarrhœal Affections in Copenhagen, during the Six Years, 1863—68 inclusive. (According to the Weekly Lists of Morbidity sent in by the Physicians of Copenhagen.)*

Diseases.	1863.	1864.	1865.	1866.	1867.	1868.	Yearly average.
Typhoid fever .	1,620	1,989	2,808	2,033	1,895	2,437	2,130.3
Cholérine and diarrhœa . .	810	927	2,242	1,178	2,800	5,419	2,220.3
Asiatic cholera	4	0.6
Dysentery . .	34	38	37	27	25	30	31.8
Total . .	2,464	2,954	5,087	3,242	4,720	7,886	—

¹ The first of these tables is deduced from the data furnished from the original detailed records belonging to my office; the second is, on the contrary, derived from the published communications of the Statistical Department. It will be noticed that, while in my list Asiatic cholera is found entered in 1866 and 1867, with only seven fatal cases in all, of which but *three* are reported from the city itself, that of the department (for the provincial towns) includes several entries for each year of the ten, which is also the case in the list for Copenhagen furnished by the same body. This must, however, be due to a mistake; at least I would venture to assert, with tolerable certainty, as to Copenhagen, that within the last ten years (1860—69), no case of Asiatic cholera has occurred there, except in 1866. We become the more convinced of this if we examine the details, in which we find that those reported as dying from cholera in other years than 1866 had for the most part been either children in their first year, or persons at a very advanced age. The mistake has probably arisen from a deficiency in the principal form in use amongst us, in which the separate causes of death are entered, as this, for instance, has no heading for cholérine, a malady which (as is well known), in weakly children and delicate persons of great age, may often be followed by death.

TABLE VI.—*The Number of Patients treated for Diarrhœal Affections in the Kingdom of Denmark, excluding Copenhagen, during the Six Years 1863—68 inclusive. (According to the Tabular Reports given in by the Official Physicians to the College of Health at the close of the Year.)*

Diseases.	1863.	1864.	1865.	1866.	1867.	1868.	Yearly average.
Typhoid fever .	9,028	12,369	13,540	10,523	8,158	14,863	11,413·5
Cholérine and diarrhœa .	1,815	2,621	5,603	2,552	5,811	11,645	5,007·8
Asiatic cholera	1	0·17
Dysentery .	134	524	200	156	137	168	219·8
Total .	10,977	15,515	19,343	13,231	14,106	26,676	—

From a glance at all the tables it will readily be seen that diarrhœal affections have been of especially frequent occurrence in 1865 and 1868, not so, on the contrary, in the cholera year 1866. In 1865 we had in this country an unusually heavy snow-fall during the winter and till late in March, a damp spring, and an uninterruptedly warm summer. If the sun's action on a soil with a high and also quickly sinking level of the subsoil water should be capable of causing a spontaneous development of cholera (without any communication whatever of exotic contagion), then all the conditions are seen to have existed in this country in 1865. We, however, meet with no cholera (just as little as in 1866), but with frequent cases of diarrhœa and cholérine, which also happened in the unusually warm and dry summer of 1868. With reference to 1864, it should be remarked that as far as concerns Denmark this was a war-year, and that therein may be sought the reason of the great prevalence of typhoid fever compared with the immediately preceding years. Undoubtedly, among these cases, many of exanthematous typhus have been reckoned—as it is said also happened in the first war in Schleswig, for example among the patients treated in the Augustenberg Hospital—but it is only very recently, indeed, that the attention of the Danish physicians has been for the first time directed to this form of typhus, since, as already mentioned, typhoid fever had hitherto exclusively prevailed in this country.

Lastly, as regards most recent circumstances, it should be mentioned that although the cholera has been epidemic in St. Petersburg, and at some Finnish ports, since the middle of August, 1870, yet Copenhagen has been so far spared from any

new cholera invasion from the frontier. Still, in August, 1870, a suspicious case occurred in this city, in which death ensued within twenty-four hours, and which was regarded by the attending physician as one of Asiatic cholera. The occurrence of this isolated case is the more remarkable from the fact that the patient was a prisoner in the House of Correction, and also that the disease attacked him in a completely isolated prison (namely, in the large gaol situated at Christianshavn, a suburban quarter of the city). Appropriate measures were at once taken on the part of the medical officer of the institution to isolate the patient, to effect necessary disinfection, &c., and no fresh case subsequently happened. Meanwhile it is worthy of attention that Dr. Hempel, the medical officer of the institution, has written to me about this suspicious case to the following effect:—

“A remarkable fact is that the prisoner who was attacked had just at the time, curiously enough, uninterrupted intercourse with persons outside the prison, as he was doing duty as wall-hodman for the workmen who were then employed in the erection of a new range of cells. The prisoner, who during his illness attended the patient, and who at other times worked and slept in an isolated place, had immediately afterwards an attack of diarrhœa, which prostrated him much, though he recovered.”

It may be remarked that there exist at Christianshavn a large number of lodging-houses for foreign seamen. It has been fortunate for the city that the marine navigation, owing to the war, was less active than usual, and that so only one case of cholera was brought into the roads. The patient, a sailor bound for another port, remained on board. As far as we can conclude from the medical periodicals, neither in Sweden nor in Norway has any case, so far as I am aware, of Asiatic cholera occurred either in 1870 or in the present year.

In conclusion, I should not omit to refer to some numbers of the ‘*Ugeskrift for Læger*,’¹ in which Journal there has appeared for the last five years a *short review* of the “State of Morbidity at Copenhagen,” especially in an epidemiological point of view. These reviews, which had previously been communicated by the Hygienic Committee of the Royal Medical Society, have since 1868 been drawn up by the Medical Officer of Health to the city.

COPENHAGEN; *May 8th*, 1871.

¹ ‘*Ugeskrift for Læger*,’ March 9th, 1867; April 25th, 1868; July 24th, 1869; and October 15th, 1870.

APPENDIX No. 1.

*Law respecting the Repeal of the Quarantine Precautions against Yellow Fever and Asiatic Cholera.*CASTLE OF CHRISTIANSBORG, *March the 10th, 1852.*

WE, Frederick the Seventh, by God's grace King of Denmark, the Vends and Goths; Duke of Schleswig, Holstein, Stormarn, Ditmarsh, Lauenburg, and Oldenburg:

Make Proclamation: the Diet has enacted, and We by our consent have ratified the following law:

(1.) For vessels bound for Danish ports, the quarantine against yellow fever is remitted in the case of both persons and goods.

(2.) Likewise all quarantine against Asiatic cholera is remitted; nevertheless, it is to be observed that when vessels arrive with cholera patients, or with the bodies of the same on board, such vessels are kept isolated, until the matter is notified before the Quarantine Committee or the Sanitary Police of the place, which bodies, when the proper treatment of the sick can be arranged in a suitable locality, must give permission for their landing, with due regard to the necessary precautionary regulations.

In places where sanitary and river dues are paid, no cholera patient can be brought ashore so long as cholera is not prevalent there.

The closing of islands and provinces authorised by the Rescript of October 14th, 1831, is repealed.

(3.) Vessels which only touch at Danish ports, but are bound to a foreign Baltic port, should as heretofore be reminded to observe the quarantine directions in force for the respective harbours, the maintenance of which the Danish Government has taken upon itself, and which contain the stipulations for obtaining the Danish quarantine-pass or pratique.

All earlier arrangements and regulations which are at variance with the conditions contained in the first and second paragraphs of this law are hereby rendered invalid.

Whereto all concerned must conform.

Given at Our Castle of Christiansborg, March the 10th, 1852, under Our Royal Hand and Seal.

FREDERICK R.

APPENDIX No. 2.

Law respecting the Measures for Preventing the Introduction of Asiatic Cholera by Sea.

COPENHAGEN, May 1st, 1868.

WE, Christian the Ninth, by God's Grace King of Denmark, the Vends and Goths; Duke of Schleswig, Holstein, Stormarn, Ditmarsh, Lauenburg, and Oldenburg:

Make Proclamation: the Diet has enacted, and We by our consent have ratified the following law:

(1.) No vessel arriving at Copenhagen, or any other seaport in the kingdom, from any place where, according to a notification from the Minister of Justice, Asiatic cholera is assumed to prevail in an epidemic form, is permitted to land any person on board (passengers or crew), before it has been examined by a physician under the direction of the Committee of Quarantine or the Sanitary Police.

The same holds good in respect of vessels arriving from other places, which on the voyage have had on board either patients suffering from, or the corpses of those that have died of, cholera.

(2.) The vessels specified in paragraph (1) are required on their arrival in the roads, and before entering the harbour, to hoist the quarantine flag, or in default of it, a white flag on the topmast.

(3.) Provided that on medical examination it is found that the vessel has, or on the voyage has had, suspicious cases of disease (sick or dead) on board; or that its state of health on the whole gives occasion for apprehension of the introduction of infection, the Committee of Quarantine, or the Sanitary Police, must, before any person on board is landed, enforce the requisite precautions with respect to the sick, to the removal of the dead from the vessel, and to its disinfection; and the vessel is kept isolated until these measures are carried out.

(4.) In Copenhagen and the seaports there shall be had ready at hand, as often and for so long time as the Minister of Justice shall consider necessary, a suitable place into which those suffering from cholera may on their arrival from sea be received, and undergo appropriate treatment. In seaports where such places do not exist, they are established by arrangement between the governor of the district and the municipal council of the place in question. When the circumstances require a speedy decision, the municipal council of the place undertakes the necessary measures of precaution.

(5.) For the examination mentioned in paragraph (1) there

is due to the physician a fee of two rix-dollars (*i.e.* four shillings and sixpence) for vessels of sixty tons and under, of three rix-dollars (six shillings and ninepence) for vessels up to a hundred tons, and of five rix dollars (eleven shillings and threepence) for vessels over a hundred tons. The fee to the physician, and his travelling expenses to and from the vessel, as well as the costs of the attendance on such patients as are brought on shore, and of the disinfecting of the vessel, are defrayed by the vessel, which is arrested for the amount, and which, before any of those on board are landed, must give security for the payment of the charges whereto it becomes liable. All other expenses depending on the carrying out of this law are defrayed at Copenhagen by the city treasury; in Bornholm by the public fund of the market towns and country districts of the island; and elsewhere by the departmental fund in each official district, as also by the market towns in each such district in proportion to the population.

(6.) The infringement of paragraphs (1) and (2), and of such provisions as the Committee of Quarantine, or the Sanitary Police, may enforce with reference to paragraph (3), is punished, so far as no severer penalty is inflicted in virtue of the general legal code, by a fine of a hundred rix dollars (eleven pounds and five shillings), which reverts to the state treasury. Cases of infringement of this law are treated as public police cases.

(7.) The Minister of Justice is authorised by a decree, which is published in the usual way, to make the provisions of this law applicable to other dangerous epidemic diseases than that named in paragraph (1), and to extend the operation of the law to wharfs or other places outside the market towns to which vessels come.

Further, the Minister of Justice is authorised, when circumstances require, to grant remissions in the instructions contained in paragraph (1), and to fix the medical fee for investigations made on shore.

(8.) This law is not in force in the Farøe Islands.

Whereto all concerned must conform.

Given at our Palace at Copenhagen May 1st, 1868, under our Royal Hand and Seal.

CHRISTIAN R.

III.—On Puerperal Mania. By THOMAS MORE MADDEN, M.D., M.R.I.A., Ex-Assistant Physician, Rotunda, Dublin Lying-in Hospital; M.R.C.S., Eng.; L.K. & Q.C.P.I.; L.F.P. & S., Glas. Formerly Demonstrator of Anatomy, Carmichael School of Medicine; Corresponding Fellow of the Obstetrical Society of Edinburgh; Corresponding Member of the Gynæcological Society of Boston, U.S., &c.

PUERPERAL mania is one of the most formidable, and not least frequent, diseases of childbed. Having had an extensive opportunity of studying this malady in the great maternity hospital with which I was connected, I now submit the following observations, made for the most part at the bedside, in the hope that they may be considered of some practical interest.

In ordinary private midwifery practice puerperal mania is by no means so frequently met with in proportion to the number of cases attended, as it is in the practice of a large metropolitan lying-in hospital. The reason for the difference in this respect between private and hospital obstetric practice is, that in the former the patients are generally in better circumstances and social condition, having less mental anxiety and physical privation than in the latter, especially if the public institution be one where every parturient woman who seeks admission is received without question. In such an institution the majority of patients are married women of the poorest class; wives of labourers and artisans, often broken down physically, and depressed mentally, by poverty and hardship. But besides these, there are also admitted a considerable number of unmarried women not unfrequently the victims of seduction, who come into the hospital to seek a shelter in the hour of labour, hoping at the same time to hide their shame and pass unnoticed in the crowd of patients. The records of the hospital prove conclusively that these latter are the most frequent subjects of puerperal mania.

During labour, at the termination of the second stage, when the child is passing through the vulva, there frequently occurs a paroxysm of high mental excitement, which some describe as a form of puerperal mania, but which is merely the transitory delirium of intense pain. This symptom is very commonly observed during labour, and is too transitory in its effects to require any special notice.

As the term puerperal mania has been applied to cases which I do not consider within the scope of this paper, I may, in the first place, observe that the subject of this communication is the etiology, pathology, and treatment of mental derangement occurring as a consequence of parturition, and within the period during which the

physiological changes which take place in the uterus and its appendages after delivery are still going on.

I am of course aware that by many writers on the subject the term puerperal mania is understood to include those forms of mental disturbance which occur occasionally during pregnancy; and also that more common cerebral affection which sometimes results from over protracted lactation. These cases, however, I regard as distinct affections from that which forms the subject of the following observations:

Etiology of puerperal mania.—The causes of puerperal mania may be divided into predisposing, exciting, and proximate. The last term, although now so generally discarded, I have still retained, as it appears to me to express most appropriately the immediate, or essential, physical change, which, as will presently be seen, I regard as intimately connected with the malady now under consideration.

In many of the cases of puerperal insanity which have come under my observation, no predisposing cause was ascertained to exist. But in others, and these the larger number of cases, the disease in question was evidently connected with physical want and mental distress. Previous mental disease and family predisposition to insanity had existed in several cases.

The average age of the patients affected with puerperal mania corresponds very closely with the period of life at which pregnancy is most frequent. Thus, of 1996 cases of puerperal mania which I have collected from various authorities, in 1239 cases the disease occurred between the ages of twenty and thirty. The number of primipara attacked by puerperal mania is greater in very young and very old women in proportion to the total number, and more especially so in persons advanced in life. The same tables also prove that primipara are more liable to puerperal mania than multipara.

The condition of the patient as to marriage appears to have a marked influence in the causation of this disease. A large proportion of the cases of puerperal mania occur in unmarried women. Thus, of the patients suffering from mania after parturition that I have seen, twelve out of twenty were unmarried. The same fact is established beyond doubt by the statistics of the disease, as may be seen by the following table:

Authority.	Total cases.	Married.	Unmarried.	Widows.
Hanwell report	415	122	263	30
Queen Charlotte's Hospital report	11	3	8	
Dr. Jacobi's report . . .	835	156	599	80
Dr. Tuke's report . . .	73	60	13	

The explanation of this circumstance is very obvious, and to it

may be applied the reasons by which I have elsewhere accounted for the prevalence of puerperal fever amongst unmarried patients. In their case the pangs of labour are assuaged by no moral consolation; the present is full of anguish, and the future is lighted by no ray of hope. Can we wonder that these poor creatures, predisposed to disease by the combination of every moral as well as physical cause which could depress their vital powers, indifferent to life, nay, even, as I have but too often heard them, wishing for death—should, under such circumstances, be peculiarly liable to puerperal mania as well as to metria.

The pregnant state itself appears to predispose to functional cerebral disturbances in many cases. To this fact may be referred those otherwise unaccountable alterations in tastes, habits and dispositions, that irritable condition of mind and temper, those unreasonable likings and aversions, those irresistible longings and foolish fancies which in some women invariably accompany pregnancy.

In several of the cases of puerperal mania which occurred in the practice of the hospital, and more especially in the extern practice, the shock and exhaustion of difficult or complex parturition appeared to act as the exciting cause of the disease.

In cases where puerperal mania is attended by any other puerperal disease affecting the whole system, such as puerperal fever or any of its varieties, metria, pyæmia, &c.; or where less serious puerperal affections accompany the mania, such as milk fever, hysteria, or ephemeral fever, the mania is merely symptomatic of the morbid constitutional condition of the patient, even though it may somewhat precede any manifestation of the latter. But, in the majority of cases, puerperal mania is uncomplicated by any other disease, and must be ascribed to the operation of depressing mental influences, or to the nervous shock and exhaustion consequent on parturition, or to the combined action of both causes. The nervous system being at this time in a state of peculiar tension, and the physical condition of the patient being one of depression and exhaustion, it is easy to suppose how readily, under such circumstances, the puerperal state may act as the exciting, as well as the predisposing, cause of mania.

The ordinary exciting cause of puerperal mania is the injudicious kindness of the patient's family and friends, who too often insist on being admitted to visit her. I have seen so many examples of the ill consequences of such visits in causing mental excitement that, as far as possible, I now exclude all visitors from the lying-in room until the patient is able to sit up. For the same reason all unpleasant news, or any conversation which might in any way excite her, must be avoided. I have known puerperal mania follow from a patient being allowed by an injudicious nurse to read a letter containing some unexpected family intelligence on the eighth day after delivery. With few exceptions, the cases of puerperal mania that I have seen

manifested the symptoms of the malady within the first week after delivery; and this fact is to some extent a confirmation of the old idea that the complaint in question is connected with the irritation caused by the secretion of milk, especially if this be disturbed or put back, in which case a metastasis to the cerebral system was held to take place. Dr. Horatio Storer, of Boston, has, in his recently published work on 'Reflex Insanity in Women,' with great ability advocated the view that the frequency of insanity in females is owing to reflex irritation caused by ovarian or uterine derangements. The same theory has been applied to explain the cause of puerperal mania. There can be no doubt, however, that toxæmia, or puerperal blood poisoning, plays at least fully as important a part in the causation of puerperal mania as reflex irritation does. In the great majority of these cases suppression, or diminution of the lochia, which generally became fœtid as well as scanty before disappearing, was observed. The retention of the matter which should be thus eliminated, and its circulation through the cerebral vascular system affords, I think, a not improbable explanation of many of the phenomena of puerperal mania. I must say, however, that I have seen well marked cases of puerperal mania in which there was no diminution of either the mammary secretion or the lochia.

To understand why it is that puerperal women are so liable to a peculiar and generally speaking transient form of mental disturbance, we must bear in mind that for healthy thought, or mental action, a healthy condition of the blood in circulation through the brain, as well as a normal state of the circulation itself—that is to say, a perfect freedom from any derangement of the general circulation, or local misdirection of blood which might seriously disturb the balance of the circulation, is essential. I have seen more than one case of puerperal mania which commenced apparently during the patient's sleep; and this reminds me of an observation I made in an essay of mine published some years ago, 'On Dreaming considered especially in relation to Insanity,' viz. that fearful dreams, if frequently repeated, may eventually influence the permanent state of the mind; and, considering the close resemblance between the phenomena of dreaming and insanity, which is but "a waking and active dream," we may expect that the former condition, if prolonged, might pass into the latter state. Insanity occasionally does commence in a dream that continues after the cessation of sleep, and cases are recorded in which persons recovering from mental alienation were nightly disturbed in their dreams by the same hallucinations which had previously haunted them in the waking state. "Some women, for instance," says Dr. Storer, in his recent work on 'Reflex Insanity in Women,' "are much troubled with frightful dreams whenever they are pregnant. Dr. Lowder used to relate the case of a lady who was obliged to have a nurse sitting at her bedside all night to

watch her countenance while she slept, and to awaken her as soon as she perceived her exhibiting distress under the influence of her dreamy terrors." This fact did not escape the notice of the most observant obstetric authority of the last century, in whose description of the phenomena of the disease it is alluded to, and therefore I have thought that the following extract from a manuscript report of Dr. William Hunter's Lectures on Midwifery, might be of interest. These lectures, which were reported by some member of Dr. Hunter's class during the session of 1785, are now in the possession of Dr. Johnston, the present distinguished Master of the Rotunda Hospital, Dublin.

"*Lecture XLI.* The next disease of the puerperal state to be mentioned is Phrenitis. When, upon paying our second visit, we find that the patient has had no sleep, that her pulse is not less than 100; when we inquire of her how she is, she replies that she has no complaint; we observe a remarkable quickness of her speech, her sight and hearing are also very acute, as, indeed, are all the senses to external impressions. If the lochia are present they go on naturally: the appetite remains normal, as also does the belly; in fact all the functions are now natural. In a few hours, however, violent pain in the head comes on, and often active outrageous delirium. In all cases wherein Dr. Hunter attended the disease came on in forty-eight hours after delivery. *Sometimes it attacks the woman while sleeping and dreaming.* Light and noise now make little impression. The pulse becomes more frequent, the excretions remain natural, the pupil becomes dilated. Whatever is offered her she readily swallows; the countenance remains natural, and so outrageous does she become that she frequently endeavours to leave her bed, or to spit upon those around her. Her pulse still increasing in frequency; stertorous breathing comes on, and now for the first time the attendants become alarmed for her safety. The countenance now becomes changed, the features sink, the eyes covered with mucus, and sometimes subsultus tendinum take place, sometimes also convulsions, and she at length sinks."

Bearing in mind the foregoing observation of Hunter's, the reader may understand my reason for applying to the explanation of what the old writers termed the *proximate cause* of puerperal mania a theory of my own, by which I attempted several years ago to explain the phenomena of dreaming. For it appears to me by no means improbable that the transitory mental disturbance of puerperal mania, to which more properly than to any other form of mania might be applied the observation that insanity is but a waking and active dream, is due to a similar proximate cause to the latter condition. In the communication just referred to, read before the Medical Society of the Dublin College of Physicians, I endeavoured to prove that dreams are caused by a

partial relaxation of the tonic contractility of the cerebral veins, which limits the amount of blood that passes through these vessels, and that thus the different parts of the encephalon may be in very different states of vascularity at the same time. For instance, the blood may be moving with much greater force and rapidity through the capillaries of the base of the brain than in those of the superior portions of the hemispheres; or, in other words, that something like active congestion, confined to a small portion of the cerebrum, occurs. And if, as it has been conjectured, the different parts of the brain are distinctly subservient to the several functions of the mind (a point, however, which can by no means be regarded as settled); and if, moreover, we admit that the local cerebral congestion and accompanying derangement in the balance of the cerebral circulation already spoken of may be due to local irritation produced by the action of puerperal blood-poisoning, by retained and morbidly altered lochial and other excretions that have been suppressed or diminished and decomposed so as to act as zymotic blood poisons; by this theory we might more easily comprehend the phenomena of a state in which certain of the mental powers are unduly excited and active, whilst the other faculties of the mind are for the time either blunted, perverted, or entirely suspended.

Varieties and symptoms of puerperal mania.—Two distinct forms of puerperal mania have come under my observation, viz., 1st, violent mania, attended with symptoms of inflammation of the brain or of its meninges; and, 2ndly, a low form of mental disturbance, in some cases presenting the symptoms of melancholia, in others, and much more commonly, closely resembling traumatic delirium. The acute inflammatory form is generally earlier in the period of its occurrence after labour than the low desponding type of the disease, and is, moreover, generally more favorable in its prognosis, as far as the mental condition of the patient is concerned. Two subdivisions of the disease were also noted. In the first the mental disturbance was not accompanied by any well-marked bodily complaint. In such cases it was observed that the pulse was, generally speaking, considerably accelerated, and I may here note the fact that in this disease, perhaps, more than in any other form of mental disturbance, the rapidity of the pulse may be regarded as a fair criterion of the severity of the attack. To this rule, however, I have seen some striking exceptions. The pulse in such cases generally approaches 100; when it is over 100 the case is very serious, and in the worst cases I have seen the pulse was over 120, and of these patients one died. In the form of the disease we are now considering the patient's face was generally pale, her skin cool and moist, and no headache was complained of. The accompanying delirium was usually of a low muttering character, the patient was anxious and desponding, and her condition was very similar to that of a person in typhoid

fever. It was often afterwards ascertained that patients thus affected had been victims of seduction.

In the second class of cases the disease sets in with decided evidences of pyrexia, and symptoms which might mislead an unwary practitioner into treating the case as one of active inflammation of the brain or of its membranes, requiring bold antiphlogistic treatment. In this form of puerperal mania there is usually a premonitory stage of sleeplessness and irritability of manner observed, the patient's mental powers become unduly active and her perception quicker than natural. Headache is generally complained of, she gradually becomes more excitable in manner; frequently appears in very high spirits, laughs loudly and causelessly, talks loudly and with a peculiarly rapid articulation. As the disease advances all these symptoms increase. The talking becomes a loud incessant babbling, generally on the subject of some imaginary injury or affront. Maniacal violence succeeds to mere irritability of manner, and is commonly directed against those whom the patient naturally holds dearest to her. Not unfrequently a homicidal tendency exhibits itself, having for its special object the destruction of the child; and if prevented from this by proper precautions, the patient, if unwatched, will probably escape from bed and may attempt self-destruction.

It is of great importance to watch for and detect the premonitory symptoms of puerperal mania, for thus detected the approaching disease may oftentimes be warded off by proper treatment. I have seldom seen a case of this kind which was not ushered in by a premonitory stage of insomnia, quickness of pulse, and an alteration in the patient's manner, which generally became discontented and quarrelsome with those about her. In some few of these cases, however, the disease appeared to commence suddenly, without any premonitory symptoms being noticed. In one case the patient awoke suddenly delirious, having been frightened in a dream, and having been apparently well when she went to sleep. In another the disease was ushered in by an attack of epilepiform convulsions, on the subsidence of which the patient was found delirious; and in a third, a determined attempt to kill the child was the first thing to attract attention to the patient's mental condition.

I may here observe that a decided aversion to some person who, if in her normal state of mind, should be dearest to the patient, and more especially to her child, was the most common and most prominent symptom, as far as my experience goes, of this disease.

Erotomania and obscenity were not very commonly observed in these cases. But in some of them obscene ideas and expressions appeared to have entire possession of the patient's mind. In one case I attended, a young lady of high social position, of remarkably religious habits, and of the purest life, whose insane salacity of

thought found expression in words which it was difficult to believe that she could have ever heard.

In most cases of puerperal mania marked derangement of the digestive functions was noticed; the patient's tongue was furred, the appetite either impaired or preternaturally large; her breath offensive, her bowels constipated, although in some cases, and these the worst, they were too free, the motions being passed unconsciously, and the evacuations unhealthy and fœtid.

Obstinate silence was a striking feature in two of my cases, the patients refusing for some days to reply to any questions, although it was obvious that they understood the observations made in their hearing and the questions put to them.

The pathology of this disease, as illustrated by post-mortem examinations, is still very obscure. The majority of cases recover; and of those who do not recover, a large proportion fall into a state of chronic insanity or dementia, and die of other causes than puerperal mania, and so Esquirol, who had numerous opportunities of making post-mortem examinations in the vast lunatic asylum under his charge, acknowledges that of the cases of this kind in which he examined the brain after death, he was unable, from the morbid appearance, to point out the cause or seat of the disease. In the majority of these cases some cerebral congestion was observed, and in the only immediately fatal case that occurred during my connection with the Dublin Lying-in Hospital, the only morbid appearance in the brain was a slight thickening and a vascular condition of the arachnoid, with considerable subarachnoid serous effusion, the exact amount of which was not measured, but which was about six or eight drachms.

Prognosis in cases of puerperal mania.—With regard to the probable result in such cases, there are two distinct questions to be considered:—1st. The probability of death from the disease; and 2ndly, the likelihood of the malady eventuating in permanent insanity.

The majority of cases of puerperal mania terminate in recovery; the next most frequent result of the disease is death from it, whilst the least common termination is in confirmed insanity.

Thus summing together all the cases of which the termination has been recorded of puerperal mania that I have either observed myself, or met with in the course of my reading on this subject, I find that out of every 1000 cases of puerperal insanity 668 recovered within six months of the attack.

The following table, framed from the reports of the most eminent writers on this subject, shows clearly the tendency of puerperal mania to terminate favorably:

Authority.	Total number of cases of puerperal mania.	Recovered.	Died.
Esquirol	92	55	6
Haslam	85	50	—
Burrows	57	35	10
Webster	181	81	—
Hardy and McClintock	7	6	—
Gooch	13	9	4
Johnston and Sinclair	26	18	3
Tuke	73	56	6

Treatment of puerperal mania.—With regard to the treatment of puerperal mania the indications are, 1st, to allay the nervous irritation; and 2ndly, to support the patient's strength, recollecting always that this is generally a disease of an asthenic type.

In most cases it was found necessary to commence the treatment by removing any source of irritation from the *prima via* by purgatives or laxative enemata, as the case might require.

It has been before observed that puerperal mania is usually ushered in by diminution, and sometimes by complete suppression of the milk and lochia, and I have attempted to explain some of the phenomena of this malady by supposing it to be the effect of the circulation through the brain of a blood poison caused by non-eliminated and altered excrementitious matters. Hence our first effort in such cases should, I think, be directed to the restoration of the suppressed discharges or secretions, or, failing in this, the substitution of some other channel for the elimination of the *materies morbi* from the system.

The renewal of the secretion of milk may be encouraged by applying the child to the breast or by the breast-pump. The lochial discharge may be stimulated by warm baths, poultices and stupes to the vulva, stimulating enemata, and cupping over the sacrum, or even, as recommended by some French writers, the application of leeches to the vulva.

Bromide of potassium, which possesses such marked power as a sedative in most diseases dependent on uterine irritation, was found very serviceable when the puerperal mania was of slight severity, or of the hysterical form. In such cases it was administered in doses of from ten to twenty grains every second hour; and by its use in this way I have, I believe, succeeded in warding off puerperal mania in cases in which all the premonitory symptoms of the disease had manifested themselves.

Chloral is, in my opinion, one of the best nervous sedatives and hypnotics that can be resorted to in the greater number of those cases of puerperal mania in which this class of remedies is indicated, and in which opium, hyoseyamus, camphor, and other similar medicines are either contra-indicated for reasons which I have already spoken of, or fail to produced the desired effect. In such cases I have some-

times given chloral with great benefit in procuring sleep and allaying nervous excitement. However, I should add that in some of the worst cases of puerperal mania in which I have succeeded in obtaining sufficient sleep for the patient by the use of chloral, the disease has continued unabated notwithstanding. In ordinary cases, from ten to thirty grains of chloral has sufficed to produce sleep. In some cases, however, I have administered ten grains every second hour till sleep was obtained, and in one very severe case, where this mode of giving chloral failed, I administered by enemata thirty-grain doses every third hour till sleep resulted, which was not until ninety grains of chloral had been thus given. But I would not again counsel such large doses of chloral to be given, as fatal results have been recorded from the administration of a much smaller quantity of this medicine.

Opium is a drug of less power in the treatment of puerperal mania, as far as my experience of its use goes, than might be anticipated from the analogy of this disease in its symptoms to traumatic delirium, or from the observations of eminent authorities, Drs. Gooch,¹ Seymour,² Blundell,³ Ramsbotham,⁴ Churchill,⁵ and Maunsell.⁶ In several of these cases opiates given in ordinary doses appeared to do more harm than good, and unless administered in large doses they rather increased the excitement than the contrary. Even in the largest dose considered prudent opium and its preparations, if they failed, as was sometimes the case, to produce sleep, appeared to increase the nervous excitement, the mania generally becoming more violent than before the opium was given. In some cases, especially of hysterical puerperal mania, I have seen opium act very well; but, as a rule, I think it should be used very cautiously in puerperal insanity.

Belladonna, in small doses of the fresh extract, may be serviceable in some cases, especially of hysterical puerperal mania; but it is a very uncertain medicine, and one which requires the greatest possible caution in its administration. I have seen very unpleasant effects produced by one third of a grain given every third hour.

Where the maniacal excitement runs high, tartar emetic, in small and repeated doses, is undoubtedly a medicine of great efficacy. I have seen violent delirium completely subdued in some cases, within a few hours, by the following mixture:

¹ Gooch, 'On Disorders of the Mind in Lying-in Women.'

² Seymour, 'Thoughts on the Nature and Treatment of several Severe Diseases of the Human Body,' p. 156.

³ Blundell, 'Obstetric Medicine,' p. 589.

⁴ Ramsbotham, 'Obstetric Medicine and Surgery,' p. 570. London, 1868.

⁵ Churchill, 'Theory and Practice of Midwifery,' p. 776. Dublin, 1866.

⁶ Maunsell, 'Dublin Practice of Midwifery,' p. 252. Dublin, 1869.

R Autimonii Potassii Tartratis, gr. j;
Tinct. Hyoscyami, ʒiv;
Aquæ Camphoræ, ʒviij.

Fiat mistura capiat, ʒ; quæqua semi hora.

In some cases there appeared to be a tolerance in the system of the ordinary dose of tartarised antimony, which failed to produce any effect whatever; and in one of these cases I gave the tartar emetic in grain doses. The first grain had no effect, but the second not only quieted the nervous excitement, but, moreover, produced an alarming degree of depression of the heart's action; so that I would not be inclined to adopt this treatment again without some special necessity.

Depletion was not indicated nor practised in any of the cases on which this essay is founded.

In violent mania the application of cold to the head by constantly repeated thin cloths dipped in iced water, or an evaporating lotion, was very useful in some cases where vascular action ran high. In such cases the cold application was generally grateful to the patient, appeared to soothe the nervous irritation, and to predispose to sleep.

Blisters to the nape of the neck are recommended by some writers, Ramsbotham and others, in the early stage of the disease; but my experience is not favorable to their use until the disease has passed from the acute into the chronic form, as the irritation they occasion adds but fuel to the fire if the patient be suffering the violent excitement of the acute form of puerperal mania, although they are very useful in melancholia.

As a rule, it is necessary in these cases to separate the patient from her family, and more especially from her husband and the child. This seclusion was, of course, better carried out in the hospital practice, the patient being removed to a separate ward, and better watched, than she would be in a private house.

A patient threatened with puerperal mania should never be left for a moment unwatched by a strong and experienced nurse, to guard against the possibility of her injuring either the child or herself.

In this disease, more than almost any other, it is necessary for the physician to practise the "fortiter in re" as well as the "suaviter in modo," and, though without any unkindness of manner, show the patient that it is useless to resist his orders. Once this control has been established over her, it will greatly facilitate her restoration to a sane state of mind in a sound body.

The following cases are merely given as a few examples of the symptoms and treatment of the cases of puerperal mania which have come under my observation in hospital and private practice.

Appendix of Cases.

CASE 1.—A lady, æt. 27, in her second confinement. There was some inertia in the second stage, and I delivered her by the forceps. Her previous confinement had taken place abroad, eighteen months before, and since then she had been in very delicate health. After the expulsion of the placenta there was considerable post-partum hæmorrhage, caused by inertia, which was restrained by the injection of solution of perchloride of iron, and the administration of ergot and wine.

On the following morning I found that she had not slept since her confinement; her pulse was about 80, and her manner irritable and excited. A draught containing twenty-five minims of *Acetum opii* was prescribed.

On visiting her next morning, the 22nd, her condition appeared rather worse, the draught had not produced sleep, her pulse was 100; she was exceedingly low and nervous; whilst I was speaking to her she burst into a fit of hysterical sobbing, and complained of imaginary injuries which she supposed she had received from her family, as well as from enemies who had been introduced into the house for the purpose of torturing her. I directed thirty grains of bromide of potassium in a mixture to be taken every third hour until sleep was obtained. After taking the second dose she fell asleep and slept soundly all night. In the morning (23rd) her condition was improved, the expression of her countenance was less anxious, her pulse was 80, soft, and compressible, the delusions had disappeared, but a state of great depression of spirits, with continual causeless weeping, still existed. There was no secretion of milk. The bromide of potassium was now discontinued and reliance placed on the free administration of stimulants and nourishment. She slept tolerably well that night; next morning the melancholia had entirely subsided, her spirits soon became good, and she rapidly convalesced.

CASE 2.—A. S.—, æt. 25, first pregnancy, was delivered of a living male child on the 24th of March, after an easy labour; the placenta was in a state of fatty degeneration, was retained for three quarters of an hour by extensive morbid adhesions. I removed it, but before I was sent for very considerable hæmorrhage had taken place, and she was in an almost collapsed condition when I arrived. Brandy and beef-tea were administered freely by the mouth and by enemata, and opiates were given.

On the 29th she became delirious, the mania was of a low wandering and talkative kind. She was capable, however, of being recalled to her senses a moment by being spoken to in a loud tone.

Her pulse was rapid, there was no abdominal tenderness. She continued in this state despite all treatment till April 3rd, when the delirium suddenly and entirely abated; she became quite calm, and was sensible of her previous condition: but next morning, April 4th, she was found in a state of collapse and unconscious at the time of the morning visit, and she died the same day at 11.30 a.m.

CASE 3.—S. R—, æt. 19, first pregnancy, was delivered of a living female child after a natural labour of nineteen hours, of which only two were occupied by the second stage. She was confined on the 7th of July, and went on well until the evening of the 13th, when she was suddenly attacked by hysterical mania for which no exciting cause could be ascertained. Antispasmodics and sedatives were administered, she recovered perfectly, and was discharged convalescent on the 15th.

CASE 4.—A primipara, æt. 20, who had been married a year previously to a man of very inferior station to her own, and had suddenly passed from a condition of affluence and comfort to one of poverty and privation, was delivered in No. 7 ward, on the 4th of June, 1868, of a male child, after a very easy labour, having been less than half an hour in the second stage. On the 6th she complained of slight uterine pain and her pulse was accelerated. Dr. Denham saw her, and ordered two grains of Dover's with one grain of dried soda and two grains of grey powder every third hour. On the 9th she had castor-oil and turpentine draught, but as diarrhœa came on she was ordered an astringent mixture. On the 10th she was again placed on turpentine for the same reason as before, and that evening she became excited in manner, manifested a strong aversion to the child and to her husband, for whom she expressed the greatest contempt and dislike, although he was a very fond and indulgent husband, and expressed the most kindly affection for one of the pupils and myself to him when he came to visit her. She did not sleep on the night of the 12th, 13th, and 14th, although opiates were administered to her. She gradually became worse, and on my visiting the ward on the 14th I found her exceedingly loquacious and excited. She informed me that she was a great deal better, having relieved herself by giving the child who had been incautiously left with her "a right good smacking," as she expressed it. The child was taken from her and anodynes prescribed. On the 4th the mania was now very violent, she attempted to escape from the hospital, and her incessant shouting resounded through the corridor. She was removed to a separate ward, and placed on tartar emetic and *Acetum opii* in small doses every second hour. This treatment was continued for two days without any improvement, and on the 16th she was sent to the Richmond Lunatic Asylum, where she remained for six weeks, at the expiration of which she

was discharged perfectly well. She afterwards came to see me and had a perfect recollection of everything that had occurred, whilst she was suffering from the puerperal mania. About a year afterwards her circumstances became again very comfortable, her husband got a good situation in England, and before going to join him she sent for me to attend her in her second confinement, which took place in July, 1869, and passed off very favorably without any return of puerperal mania.

CASE 5.—A. S—, æt. 35. Fourth pregnancy; was delivered in No. 12 ward, March 12th, 1870, at 8.45 a. m., after a natural labour of 9 hours and 45 minutes. She was only a quarter of an hour in the second stage, and ten minutes in the third stage. The child was a male, alive, and weighed seven pounds. On the 15th, appearing rather weak, she was put on wine and beef tea, and a stimulating draught with chloric ether, and Hoffman in camphor water was given in the evening. Next day I found her pale and anxious-looking, her pulse was about 100; she had no abdominal pain or tenderness over the uterus, the lochia were natural and there was abundance of milk; but on inquiry I ascertained that she had not slept for the last three nights. I accordingly ordered her the following draught:—

℞ Hydratis Choralis, gr. xxx;
Syrupi, q. s.;
Aquæ Menthæ Pip, ʒj;

Mist. Fiat haust statim sumend.

Wine and beef tea were given. After taking the draught she slept for three hours. At the evening visit this draught was repeated with similar effect. On the two following days she required nothing beyond wine, beef tea (which were continued as long as she remained in the hospital), and the ordinary anodyne draught of the hospital. During the night of the 18th, however, puerperal mania suddenly set in; this manifested itself at midnight by a sudden and determined attempt to kill her child, which was rescued with some difficulty. She passed a sleepless night, and next morning was ordered draughts with twenty grains of bromide of potassium. After taking the second of these, in the evening she slept for three and a half hours. But still she passed a bad night, and was delirious on the following morning, the 20th, when her tongue was dry and furred, her pulse rapid and small; was now rather taciturn. The bromide of potassium was repeated; but at the evening visit, as she appeared worse and the mania greater, and the taciturnity exchanged for excitement, Dr. Johnston ordered thirty grains of chloral, after taking which she appeared for a time in a state of great nervousness and fear, but soon however fell asleep, slept for nearly four hours, and awoke calm.

February 21st, the puerperal mania had completely subsided; she

was calm and rational; however symptoms of well-marked typhoid fever now manifested themselves, and she was sent to the Hardwick Fever Hospital.

CASE 6. (bed 62), December 14th, 1869.—F. B—, first pregnancy; was delivered of a healthy living male child by the forceps at half-past eleven p.m. She had been eleven hours in labour, four hours in the second stage and ten hours in the third stage. The head presented in the second position, and owing to this and to rigidity of the perinæum the labour was rendered difficult.

On the 15th at midnight she complained of abdominal pain and tenderness. Her pulse 108, tongue moist and respiration natural. I ordered a draught with a drachm of turpentine and fifteen drops of Acetum opii and Chloric ether, and turpentine stupes and poultices over the seat of pain. On the 16th, having still some tenderness over abdomen, the turpentine, draughts, and poultices, were repeated and beef tea was ordered. She also complained of her breasts, and the breast pump and cere cloths were directed to be applied.

17th.—When visiting the ward for Dr. Johnston I observed a slight but very peculiar expression of excitement in her face, and a rather talkative manner. After a dose of tartar emetic she became much more tranquil; her pulse fell, and at 7.30 a.m. she was so calm that I was able to leave her in charge of the nurse.

18th.—At 9.30 a.m. she was quite calm. An hour later I was hurriedly sent for, and found her in a state of depression approaching collapse; her pulse very low and fluttering, countenance shrunk, pale and anxious, skin cold and clammy. I applied a sinapism over the heart, and gave a stimulating draught. Dr. Johnston now saw her, and the sinapism and stimulant began to improve the state of her circulation; her pulse rose, and the cerebral excitement became at once increased. She clutched Dr. Johnston when about to leave so that he had to extricate himself, though with all gentleness, and having taken every pains to soothe and compose her, from her grasp. He had the child removed into another ward, and with much resistance on her part laid her down in the bed in which she had been now standing for a couple of hours. I then administered a grain of tartar emetic, which she took, having first made her preparations for death, said her prayers and obtained a solemn promise from me that I would rest satisfied with poisoning her and not add to my crime that of opening her body after the death to which I was consigning her. The tartar emetic occasioning no emesis, and appearing to calm her to some slight extent, I repeated it in an hour's time with the same difficulty as before, and induced her to take some warm drink to promote its emetic action. However, she was not even nauseated by the large dose of tartarised antimony. She complained loudly of

our taking away the child to kill it after poisoning herself. She had not slept well the previous night, her sleep having been disturbed by dreams of horrible figures which continued to haunt her for a few moments after waking and opening her eyes. She was conscious, however, that this was an illusion. I ordered her some extra nourishment, beef tea, &c., and a full anodyne at bedtime. I also pointed out the case to the class as one of commencing puerperal mania.

Next morning at 4 a. m. I was sent for to see Mrs. B—, who was in a state of high maniacal excitement. On entering the room I found the patient standing erect in the centre of the bed, almost perfectly nude, holding her infant tightly clutched to her heart, and talking incessantly at the top of her voice. I endeavoured to induce her to lie down, but in vain; and as she was shaking the child about with great violence, at the same time protesting vehemently that she would not kill it, I was obliged to take it from her, which I succeeded in doing with some difficulty. I ordered a mixture containing tincture of hyoscyamus, Hoffman and chloric ether, beef tea, &c. Half an hour after the visit I was again summoned to this patient, and found that, the nurse having against our directions turned her back to get something in another part of the room, she had sprung out of bed, thrown up the window and, before she was missed, was standing on the window sill outside and was in the act of jumping from it into the paved area below, when she was most providentially seized by the ward maid by her hair, having no clothes whatever on, having torn off her chemise, and thus held with extreme difficulty as she was struggling violently to throw herself off, until assistance was procured, when she was lifted into bed, put into a straight waistcoat and carefully watched until she was the same day removed to the Richmond Lunatic Asylum. I afterwards ascertained that she recovered very rapidly and left the asylum in a sound state of mind within ten days.

CASE 7. Feb. 6th, 1870.—An unmarried primipara, who had been delivered five days previously after a natural labour, suddenly showed symptoms of puerperal mania. As she was dressing to lie outside the bedclothes she began to shriek and clap her hands together in a hysterical manner, and after a few moments of violent excitement became obstinately silent, and refused to speak a word. I saw her immediately after the attack commenced, and ordered twenty grs. of chloral in a draught; this produced no apparent effect. She was also ordered wine and beef tea freely.

On the 7th, at mid-day, she had a well-marked epileptic fit, and on visiting the ward fifteen minutes afterwards I found her very restless, excited, and talkative, complaining of severe headache, and pulse 100. I prescribed

R Chlorali Hydratis, gr. xx;
Syrupi, ʒj;
Aque Cinnamomi ad ʒj.

M. fiat haustus statim sumendus.

After taking this draught she immediately dozed off, and slept for nearly four hours, when she awoke in a stupefied condition, eyes wild and staring, would not speak, decubitus dorsal. She now had hot wine and beef tea at short intervals.

8th.—Her condition continues much the same as yesterday. Continues obstinately silent, pulse small and very rapid, lying on her back kicking at the bedclothes, features pinched, expression of vacuity. Was given twenty grains of chloral at morning visit without any effect whatever. At night had thirty minims of tincture of opium in an enema, with brandy and beef tea, and only slept for one hour all night.

9th.—No change in condition or treatment.

10th.—Slept from last night till 4 a.m. She is much more talkative, and has passed from a state of morose taciturnity into one of extreme garrulosity and excitement. Visiting for Dr. Johnston, I gave her thirty grains of chloral at 10 a.m.; this at once quieted her and produced four hours' sleep. At bedtime a similar dose of chloral was also given. After taking the last draught she fell asleep within a few minutes, and for the first time since her confinement had an entire night's uninterrupted sleep.

11th.—She appears quite stupefied this morning; refuses to speak or to take food, wine, or medicine. Was sent to Richmond Lunatic Asylum.

CASE 8.—An unmarried woman, aged about 40, was delivered of her second child in No. 12 ward, March 30th, at 10 a.m.; she was in great distress of mind, exhibited all the premonitory symptoms of puerperal mania, talked wildly, did not sleep for three nights; got out of bed and insisted on going home the day after her confinement, and manifested an aversion to the child. On the evening of April 2nd she got twenty grains of chloral, and half an hour after she fell asleep and slept for the first night since her admission into hospital before delivery. Next morning she was quite free from any symptom of puerperal mania.

CASE 9.—A. S.—, aged 35, fourth pregnancy, was delivered at 8 a.m., February 12th. Her labour lasted nine hours, and was natural. The child was a male, and was living. On the third day after delivery (the 15th) she complained of great weakness, had no appetite, looked pale, and was ordered draughts, with ammonia, chloric ether, and Hoffman. On the following day her pulse was 100, the abdomen was free from pain or tympanitis; the milk and lochia were abundant and natural. She had not slept, however,

since her confinement. Beef tea and wine were ordered, and I prescribed a draught with twenty grains of chloral, which produced three hours' sound sleep, and was again repeated that night.

On visiting her on the 19th, it was found that she had had a sudden attack of violent puerperal mania during the night, but made no attempt to destroy the child. I directed twenty grains of bromide of potassium in a draught; was repeated in the evening, and seemed to quiet her, as she had three or four hours' sleep. On the 20th she was more composed, but still maniacal, and was ordered thirty grains of chloral at bed time. An hour after this draught she slept for nearly four hours, and awoke in a very nervous excited condition, but soon got calm. On the 21st the puerperal mania had completely subsided; she was quite rational, but shortly after typhoid fever set in, for which she had to be removed to the Hardwick Fever Hospital.

CASE 10.—March 5th.—I was called, in consultation with Dr. O'Neil, to see a lady, æt. 21, who had been fourteen days previously confined of her first child, and had afterwards progressed favorably till the tenth day after delivery, when she was suddenly attacked by acute puerperal mania of a very violent type. When I saw her *she had not slept for six nights*; was highly delirious; pulse 140, and weak, as she refused to take food. Two drachms of hydrate of chloral with the same quantity of bromide of potassium, and four drachms of tincture of hyoscyamus in a six-ounce mixture, an ounce of which was to be given every hour, was prescribed. Next day we found her worse; she had spit out the medicine, and had not slept nor taken food since. I now proposed to try the effects of chloral in a way in which I believed it had never been previously used, and we accordingly gave her enemata of brandy and beef tea, with thirty grains of chloral in each, every third hour. The third enema produced immediate effect, and after taking it she had eight or nine hours' uninterrupted sleep, the first repose she had had for seven nights. On awakening, however, the mania still continued with unabated violence, and we found her no better next morning. The chloral was continued in the same way till the 9th, when, finding that the disease remained without any improvement, although the medicine produced sufficient sleep each night, we determined to try the effect of a complete change of scene and air, conjoined with proper care, and for this purpose she was removed to a lunatic asylum, from which she went home perfectly cured in about three months.

CASE 11.—February 10th, 1870.—I attended a lady, æt. 24, who was delivered of a living male child at 9.30 p.m., after a natural labour of seven hours. It was her second confinement. Her last labour had occurred fifteen months before, and was followed by a

very severe and protracted attack of puerperal mania. She was a person of a very nervous, hysterical temperament, who had been indulged in every way by her parents as well as by her husband, and of a very passionate disposition. She went on well till the 13th, when her pulse was 100. She had not slept the night before, but complained of no pain or uterine tenderness.

14th.—She has now not slept for two nights, complaining of no pain; pulse 100; pupils contracted; insists that she is quite well, manner wild and excited; had got into a great rage with the nurse, and struck her for not settling the pillow properly. Ordered,

R Potassii Bromidi, ʒj;
Tinct. Hyoscyami, ʒiij;
Tinct. Lupuli, ʒiij;
Aquæ Camphoræ ad ʒviij.

M. ʒj every sixth hour.

15th.—Still continues in the same state as yesterday; increased the dose of bromide of potassium to thirty grains every sixth hour.

16th.—Slept well last night. Is calm and rational, and convalesced rapidly from this day.

IV.—On Revaccination. By P. M. BRAIDWOOD, M.D.

A favorable opportunity was afforded me, during the recent epidemic of smallpox in Liverpool, of studying the subject of revaccination, and, as far as my time would permit, I availed myself of the occasion. It was not possible in every instance to note carefully the results, but I was able to do so in 323 of my cases of revaccination; and in 300 of these cases I also carefully marked the character and number of the cicatrices which had resulted from primary vaccination. This series, which I now submit, appears to me to afford data which, so far as I know, have not yet been furnished.

The value of revaccination as a means of protection against variola, and of checking its progress when epidemic cannot be overrated, when we reflect that in this country not a single instance (to my knowledge) of smallpox after *revaccination* has been published.¹ The experience of various districts, towns, and institutions in England, which have been attacked with smallpox in the course of the last twelvemonth, confirms the observations, made over and over again during the past quarter of a century in all parts of the globe, regarding the influence of *revaccination* in arresting the progress of a smallpox epidemic.

It has been remarked, moreover, during the recent epidemic, that revaccination was successful in a large proportion of children; that the percentage of success, at all ages, was much larger than had formerly been observed; and that the type of vesicle seen in the revaccinated had a nearer resemblance to that present in the infant than used to be the case. In the accompanying tables only those cases are included in which the vesicles were found on the eighth day to present the characters of normal vaccinia. I may, moreover, remark that, although I have examined many of these cases of revaccination some months after the operation, in none have I met with a *typical* cicatrix as resulting from *revaccination*. This, no doubt, is owing to the more compact structure of the adult skin.

Revaccination, observes Dr. Seaton, "aims not only (1) at repairing whatever was irregular in the course of a primary vaccination, but also (2) at supplying what was imperfect in the *amount* of infection in cases in which the *course* of the disease was regular; and further (3), at extinguishing the susceptibility to smallpox which may remain, or may re-arise, in an indeterminate number of persons whose primary vaccination may have been complete as well as regular" ('Seaton's Handbook,' p. 269).

¹ Since these pages were written I find in the 'Brit. Med. Journ.' for Aug. 12th, 1871, p. 185, two fatal cases of smallpox recorded, and one of these patients is stated to have had "three well-marked cicatrices from early vaccination, and also three good vesicles of revaccination, which had been performed a fortnight before her admission, with lymph taken from a child."

In order to fulfil these aims it is necessary to bear always in mind Jenner's injunction—to watch the vaccinia throughout its entire course. Where this is not possible, as also in the case of children and adolescents, we can to a certain extent be guided by the number and character of the cicatrices left by the primary vaccination. Great injury has been done to the practice of vaccination by the false security which, with medical approval, has been placed in irregular or spurious vaccinia. This seems to me to have arisen from a want of due appreciation by medical men of the serious nature of vaccinia, as at once a disease itself, and yet also affording protection against disease. Vaccination is popularly regarded as a practice which it is lawful to leave in the hands of uneducated old women, and the results of which it is not necessary to observe. Hence one constantly hears of a child having suffered from smallpox within a few weeks, months, or years, after vaccination, without any notice having been taken of the type which the vaccinia assumed in the said case. I cannot forbear adding that the Registrar-General's classification of the mortality from smallpox among the "vaccinated" and the "unvaccinated" has largely tended to mislead the public as to the exact state of matters, and has greatly fostered the popular disbelief in the efficacy of vaccinia to protect against smallpox. The question is repeatedly asked by all classes, How can you prove that vaccination protects against or modifies smallpox, when the Registrar-General's returns show such a large mortality among the vaccinated? Were the smallpox mortality among the "vaccinated" subdivided into that which occurs among those under puberty and that among those beyond this age a better opportunity would be afforded for explaining the correct view of the numbers.

Granted the advantage of being revaccinated, the point to be next determined is *the proper age* at which revaccination should be performed. In this inquiry two series of data have furnished the conclusions hitherto drawn: the rate of mortality from smallpox among the unvaccinated at different ages, and the like rate among those vaccinated in infancy. In judging, moreover, of the proper age when revaccination is required in consequence of the course of the primary vaccination having been irregular, or its amount insufficient, English authorities on this subject recommend that notice be taken of the character and number of the cicatrices.

Natural smallpox, according to the statistics of Mr. Marson ('Papers relating to the History and Practice of Vaccination,' p. 19), is "most fatal in infancy and advanced life; the least so from ten to fifteen years of age; the mortality after the age of twenty rises suddenly, and increases gradually; at thirty it exceeds the mortality of infancy, and after sixty hardly any escape." In the subjoined Table III it is interesting to observe a like result exhibited. Comparing Mr. Marson's percentages with those afforded by the

successful revaccinations at various ages in my practice, I have found that vaccination succeeds best between the ages of fifteen to twenty, and fifty to sixty years. Again, in commenting on his Table V, Mr. Marson remarks, "But few patients under ten years of age have been received with smallpox after vaccination. After ten years the numbers begin to increase considerably, and the largest admitted are for the decennial period from the age of fifteen to twenty-five; and, although progressively diminishing, they continue rather large up to thirty; and from thirty to thirty-five they are nearly the same as from ten to fifteen; but as in the unprotected, at this period of life, the mortality is doubled, showing the cause to be, probably, as much or more depending on age and its concomitants, as on other circumstances. In still further advanced life, the rate of mortality will be seen to increase also, as in the unprotected state." ('Papers relating to the History,' &c., p. 21.)

The evident conclusion from these observations is, *that in cases where primary vaccination has been carefully performed and has pursued its normal course, revaccination is to be recommended, and should be performed when puberty is reached.*

If, on the other hand, it is desired by means of revaccination to obviate the susceptibility to smallpox arising from deficiency in the quality or amount (?) of the primary vaccination, how is the need thereof to be discovered? The only positive means is by exposure to the smallpox virus through contagion or by inoculation, or to vaccinia by revaccination. It is, however, held by many that the character and number of the cicatrices resulting from the primary vaccination afford sufficient data whereby to judge of the protected state of an individual. All experienced vaccinators agree in considering the superficial, indistinct scar left on a child's arm by spurious vaccinia to be indisputable proof of the insufficient protection against smallpox afforded by such a primary vaccination. In these instances the plain rule is to revaccinate *with the utmost care* as soon as the child's health will permit. It is further of the highest importance that "no practitioner should ever allow himself to neglect or overlook any means in his power of making a primary vaccination perfect, under the notion that imperfection may easily be made right by revaccination." ('Seaton's Handbook,' p. 269.) Imperfect or careless primary vaccination is a great evil, inasmuch as it produces in the minds of parents a feeling of false security against smallpox, and leaves the child exposed to a loathsome but preventible disease. If revaccination succeeds well in such a child, the practitioner is satisfied with having afforded the needed protection; but if the result is imperfect or modified vaccinia (like that following the revaccination of a properly vaccinated person), it is not necessary to repeat the operation till puberty has been attained.

It is, however, in determining the susceptibility to smallpox in the

case of a vaccinated adult that there is difficulty in forming an opinion. If, by examining in any such instance the vaccinal cicatrices, we could positively assert from their number and appearance that the individual is secured from smallpox or the reverse, this would be an invaluable criterion. The attention of medical men in all parts of the world was early directed to this inquiry; and very extensive and careful investigation of the matter has led the experienced continental vaccinators—as Fröbelius, Warlomont, and Dépaül—to declare that the character of the vaccinal cicatrix affords *no* criterion of the protection given by the vaccination. It is strange then to find Dr. Seaton stating that “the typical character of the cicatrix is our best available test, and it is a most reliable one, of the vaccination having been genuine and perfect.” (‘Seaton’s Handbook,’ p. 215.) This remark becomes still more unintelligible when we find it stated in a footnote on the same page that “foveated cicatrices, closely or exactly resembling the vaccine, may occasionally arise from other causes than vaccination; but it is rare indeed for them to be seen on the spots on which vaccination would have been performed unless as the results of that process.” Hence, in fact, according to Dr. Seaton himself, the difference between the cicatrix following vaccination and that resulting from a wound or burn is *only* one of locality. Anatomically considered, a vaccinal cicatrix is merely the imperfect cicatricial tissue covering the seat of an ulcerative process; and it indicates nothing more or less than that at a given spot the true skin has been destroyed by ulceration, and that this fictitious substitute has taken its place. The examination of many of my primary vaccinations several months after the operation leads me to the conclusion that the appearance of the vaccinal cicatrix affords no foundation on which to base an opinion respecting the character of the vaccine process or the protection against smallpox which it is likely to afford. And Dr. Ballard remarks that “persons with good and bad scars respectively stand, so far as liability to receive and develop smallpox is concerned, independently of the number of their scars, much in the same position.” If a person therefore presents characteristic vaccinal cicatrices, it is right to conclude that in this instance the ulcerative process following vaccination has proceeded uninterruptedly to a good termination; but it does not seem to me justifiable to conclude therefrom that this individual is less susceptible to smallpox than one who exhibits imperfect vaccinal scars, or that in the latter case the vaccinia has pursued an abnormal or abortive course so as to afford only an imperfect protection. The experience of all vaccinators is, that typical vaccinia is very often followed by imperfect cicatrices in consequence of the vesicles having been scratched, rubbed, or otherwise irritated, so as to delay the cicatrising process. It appears to me that too much stress has been laid, by some English authorities on vaccination, on

the statistics regarding the number and character of the cicatrices exhibited by the patients admitted into the London Smallpox Hospital, and published by Mr. Marson, to which we shall shortly refer. The results of my own revaccinations, as tabulated below, show that this process succeeded equally well among those presenting imperfect as among those who exhibited typical cicatrices. I think also that the prevailing opinion of the profession is that no reliance should be placed on the appearance of vaccinal cicatrices in those who have reached puberty. Moreover, in cases in which the cicatrix is not visible the individual is to be regarded as unprotected against smallpox; just as a person is not considered to have had smallpox except he presents variolous scars.

We next inquire whether *the number of the vaccinal scars* presented by an individual can afford any aid in judging of his security against smallpox? Jenner, Marson, Ceely, and all who have used the lymph of spontaneous or natural cow-pox, have found that it was not safe to insert it into more than two spots, and Jenner considered one group of vesicles to afford sufficient protection. On the other hand, the experienced continental vaccinators who practice "animal vaccination," make six punctures on one or on both arms, and they have met with no ill results therefrom. If one or two incisions or groups of scratches afford sufficient and complete protection against smallpox, why make four or six? One reason is, of course, that with a larger number there is a greater security against failure. The opinion that a person presenting four vaccine cicatrices is better protected against smallpox than one who shows only two, has been expressed by several English writers on vaccination; and the recommendations issued by the Privy Council to the public vaccinators in this country state that they are to "vaccinate by four or five separate punctures so as to produce four or five separate good-sized vesicles; or, it is added, "if you vaccinate otherwise than by separate punctures, take care to produce local effects equal to those just mentioned." This recommendation, as also the idea very prevalent among English vaccinators that a multiplication of the local effect secures a greater constitutional immunity, is based on statistics derived from those storehouses of information, the reports of the London Smallpox Hospital. The conclusions to be derived from the numbers therein contained are thus stated by Dr. Ballard: "The production of two vesicles is more than twice as protective as one vesicle, and the production of three or more vesicles nearly four times as protective against the casual contagion of smallpox." Comparing next the relative mortality from smallpox among those presenting one, two, three, or four and more cicatrices, Dr. Ballard observes that modified smallpox is more commonly met with in proportion to the number of the vaccine cicatrices present in any given series of cases, and that the mortality

from smallpox follows the same rule. Dr. Ballard furnishes also statistics showing that, "in those with one scar, who were *revaccinated* with success, a complete and perfect result was obtained more frequently than a modified result—a thing that is not observed in any of the succeeding series from two scars upwards; for, in the latter, either the proportion of perfect and modified results was equal, or the proportion of modified results was more or less in excess, most in excess with persons having eight scars and upwards." But in these statistics no reference is made to the ages of the re-vaccinated cases; hence we know not how many of them may have lost their protective security according to the ordinary laws of nature. Moreover, vaccination by means of two sets of scratches or incisions has hitherto been a much more general practice than vaccination in a larger number of spots. Table I, appended to this communication derived from revaccinations performed both in my private practice and at my public vaccination stations, shows the much greater proportion of persons presenting one and two vaccinal cicatrices as compared with those having a larger number of scars; and it also indicates the great success following the re-vaccination of persons possessing three and four cicatrices.

Vaccinia, from recent researches, is held to be a process excited in the human organism by a certain germ coming in contact with certain elements in the blood producing definite local and constitutional phenomena, which are believed to last till the pabulum afforded to the vaccine germ by the blood has been completely exhausted. Variola and vaccinia are, moreover, not regarded as one and the same disease, but as two closely allied diseases dependent on two separate kinds of germs. While, therefore, the constitution of an individual is in a state suitable for the reception and fructification of the variolous germ, the same person may yet not be an apt recipient of the vaccine germ. In order, however, to produce either form of disease, it is necessary to bring into contact with the blood a definite amount of the virus possessed of a certain activity. It does not matter whether this amount of germinating fluid be inserted into one or into more spots; else how is it that we find it impossible to produce the vaccine disease a second time in a child who exhibits only one group of small vesicles although vaccinated in four spots? The process indicated by this one group of vesicles affords sufficient immunity from the vaccine disease at the time, and by deduction also against the variolous disease; but how long such protection can last in a majority of cases has not yet been determined. From the revaccination statistics I have collected I believe that while one group of vaccine vesicles will protect certain individuals from smallpox for a few years only, it will be a safeguard to others up till puberty, and to a third set of persons it procures immunity for life. To state, however, that by revaccination any deficiency in the amount

of protection afforded by infantile vaccination can be made up, or that thus compensation can be procured in instances where the small number of the vaccine scars are held to indicate imperfection in the amount of the vaccinia received in childhood, appears to me to be a most misleading form of speech. On the other hand, *revaccination is to be recommended to all persons who have attained puberty and to all exposed to the contagion of smallpox*, because no one can tell regarding smallpox equally with scarlet fever, measles, or other contagious disease, whether he is "one of the wholly protected majority, or one of the but partially protected minority;" whether he is one of the many who are insusceptible to the disease, or one of the few who are prone to suffer from every contagious disease to which they are exposed.

Lastly, should revaccination be made legally compulsory? The argument we have last used, viz. our inability to determine what proportion of the population are susceptible of smallpox; as also the beneficial effect of general revaccination during recent smallpox epidemics; and the fact that by protecting all no soil is afforded to the variolous germ on which to fructify, and that, therefore, it must necessarily die from inanition,—*these are very strong arguments in favour of the enforcement of revaccination among persons congregated together*, as soldiers, sailors, students in schools and colleges, and workmen assembled in factories and other large workshops. *By careful and universal primary vaccination in childhood; by the general adoption of revaccination at puberty; and by the complete isolation of any case of smallpox whenever it occurs, may epidemics of variola be stamped out and prevented.*

Table I. shows the result of revaccinations performed in my private practice and at my public vaccination stations. In these 300 cases the ages of the persons, the number and character of the cicatrices left by their primary vaccination, and the effect of revaccinating them, have been carefully noted. Unable to find time to record all the cases, this table includes only about two thirds of all the revaccinations I have performed during the last six months. In employing the terms "good" and "indifferent" cicatrix I have adhered to Mr. Marson's definition of these words, viz. "A good vaccine cicatrix may be described as, distinct, foveated, dotted or indented, in some instances radiated, and having a well, or tolerably well, defined edge. An indifferent cicatrix, as, indistinct, smooth, without indention, and with an irregular and ill-defined edge."

Table II embraces a larger number of my revaccinations than its predecessor. In these 323 cases the ages and the result were noted in each instance; 81 of these cases having failed were revaccinated by me a second time on the eighth day after the first revaccination; and the results are here shown. The percentage of success at various ages is also stated.

TABLE I.

Age.	One good cicatrix.	One in- different cicatrix.	Two good cicatrices.	Two in- different cicatrices.	Three good cicatrices.	Three in- different cicatrices.	Four good cicatrices.	Four in- different cicatrices.	Five good cicatrices.	Six good cicatrices.	No visible cicatrix.	Nervac- enated, but had variola.	TOTALS.
Under 12 years	{}	Suc. 1 ... Fail. 1	Suc. 2 Fail. 2	Suc. 1 ... Fail. 1 Fail. 1 Fail. 1 Fail. 1 Fail. 1 Fail. 1 Fail. 1 Fail. 1 Fail. 1	Successes 4 Failures 2
12-15	{ Suc. 5 Fail. 1	Suc. 6 Fail. 1	Suc. 15 Fail. 3	Suc. 9 Fail. 4	Suc. 2 Fail. 1	Suc. 1 Fail. 1	Suc. 2 Fail. 1	Suc. 2 Fail. 1 Fail. 1	Suc. 1 ... Fail. 1	Suc. 1 ... Fail. 1	Suc. 1 Fail. 1	Successes 42 Failures 12
15-20	{ Suc. 4 Fail. 1	Suc. 8 Fail. 1	Suc. 17 Fail. 4	Suc. 10 Fail. 2	Suc. 5 Fail. 1	Suc. 2 Fail. 1 Fail. 1 Fail. 1 Fail. 1 Fail. 1	Suc. 4 ... Fail. 1 Fail. 1	Successes 50 Failures 10
20-30	{ Suc. 7 Fail. 5	Suc. 8 Fail. 4	Suc. 17 Fail. 4	Suc. 19 Fail. 9	Suc. 6 Fail. 4	Suc. 5 Fail. 2	Suc. 4 Fail. 2 Fail. 3	Suc. 1 ... Fail. 1 Fail. 1	Suc. 9 Fail. 4	Suc. 1 Fail. 2	Successes 77 Failures 40
30-40	{	Suc. 4 Fail. 3	Suc. 9 Fail. 1	Suc. 5 Fail. 4	Suc. 2 Fail. 1 Fail. 1	Suc. 3 Fail. 1	Suc. 1 ... Fail. 1 Fail. 1 Fail. 1 Fail. 1	Suc. 3 Fail. 1	Successes 27 Failures 15
40-50	{	Suc. 3 ... Fail. 1	Suc. 4 ... Fail. 1	Suc. 1 ... Fail. 1 Fail. 1 Fail. 1	Suc. 1 ... Fail. 1 Fail. 1 Fail. 1 Fail. 1	Suc. 2 Fail. 2	Suc. 1 ... Fail. 1	Successes 12 Failures 2
50-60	{ Suc. 1 Fail. 1 Fail. 1 Fail. 1 Fail. 1 Fail. 1 Fail. 1 Fail. 1 Fail. 1 Fail. 1	Suc. 3 ... Fail. 1	Suc. 2 ... Fail. 1	Successes 6 Failures 1
Totals	{ Suc. 17 Fail. 7	Suc. 30 Fail. 9	Suc. 64 Fail. 14	Suc. 45 Fail. 20	Suc. 15 Fail. 7	Suc. 8 Fail. 3	Suc. 8 Fail. 3	Suc. 3 Fail. 3	Suc. 1 Fail. 1	Suc. 1 Fail. 1	Suc. 19 Fail. 10	Suc. 8 Fail. 5	218 } 82 } 300

TABLE II.

Total Number of cases revaccinated, 323; 81 of these having failed were twice or oftener revaccinated, and yielded 23 successes.

AGE.	Revaccinated.	Twice or oftener re-vaccinated by me.	Percentage of successes.
Under 12 years {	Successes 3 Failures 5	Successes 2 Failures 3	} Successes 5 = 62·5 per c.
12—15 years {	Successes 41 Failures 11	Successes 1 Failures 10	
15—20 „ {	Successes 53 Failures 13	Successes 3 Failures 10	} Successes 42 = 80·769 „
20—30 „ {	Successes 88 Failures 34	Successes 9 Failures 25	
30—40 „ {	Successes 36 Failures 11	Successes 3 Failures 8	} Successes 56 = 81·818 „
40—50 „ {	Successes 14 Failures 5	Successes 4 Failure 1	
50—60 „ {	Successes 7 Failures 2	Successes 1 Failures 1	} Successes 39 = 82·978 „
			} Successes 18 = 94·737 „
			} Successes 8 = 88·888 „

TABLE III.

Extract from the General Board of Health 'Papers relating to the History and Practice of Vaccination,' p. 19, compared with the results of my revaccinations.

<i>Ages of Unprotected Smallpox Patients admitted into the London Smallpox Hospital, from 1836—1851.</i>									
	0—5	5—10	10—15	15—20	20—25	25—30	30—40	40—60	60—90
Percentage of deaths }	50	27	23	26	40	45	57	69	75

<i>Ages of my Cases of Revaccination.</i>							
	Under 12 years of age.	12—15	15—20	20—30	30—40	40—50	50—60
Percentage of successful revaccination at different ages }	62	80	84	79	82	94	88

Chronicle of Medical Science.

REPORT ON OPHTHALMOLOGY.

By J. F. STREATFEILD, F.R.C.S.

On the Colour of the Eyes of the Newly born.—Dr. Alfred Wiltshire had observed that it is blue, or, rather, “slaty,” and this, without exception, even in different races of men. (So it is also in the lower animals.) The “foetal” blue begins to change usually in six or eight weeks after birth. If the eyes (irides) are destined to become “dark,” *i. e.* hazel-brown or black, a brownish hue overspreads the leaden blue, and in the course of a few months effaces it. Eyes that ultimately become brightly blue, whether the shade be light or dark, gradually clear up, as it were, like the sky after a thunderstorm. The author concludes by referring to the importance of the above observations in a medico-legal point of view.—*Lancet*, Feb. 11th, 1871, p. 188.

Dr. William Ogle, on this, points out that this remark was made specially of the human eye by Aristotle.—*Ibid.*, March 4th, 1871, p. 323.

Dr. J. H. Waters attributes their coloration to the influence of light. “We know their colour is derived from its action, for the eyes and hair of the newly born are without any decided colour for days after their birth.”

(Certainly not always: the last infant we have seen just after birth had an abundance of *bright-coloured, dark-brown* hair and *grey* eyes, and now, after six months, the hair and eyes are just the same.)—*Ibid.*, April 22nd, 1871, p. 534.

Colour of the Eyes of the best Courier Pigeons.—Mr. R. W. Alldridge, at a meeting of the West Kent Natural History Society, remarked on this subject that “pearl” eyes are preferred, but that one of the most celebrated birds now in Belgium has black or bull eyes. This bird has beaten all its competitors in travelling in foggy or very heavy weather, and has reached its home late of an evening, when the other birds have lodged for the night. He continues:—“The woodcock, snipe, and all night-flying birds possess black eyes. The wild duck has a piercing black eye, and not only flies at night, but sees well under water. The swallow, so active at the close of the

day, has similar coloured organs of vision. On the other hand, the cupule, a pigeon scarcely known in England, has an eye infinitely whiter than pearl; it is the highest flying and the longest on the wing of all the pigeon tribe, and mostly chooses midday for its flight. He concludes that pearl- or white-eyed birds succeed the best in broad daylight and clear weather, and *vice versa*. One bird flew 500 miles in one day. It is said that, in taking their course, at the outset, their vision enables the best birds, in particular, to recognise some familiar landmark in relation to their home.—*Standard*, April 10th, 1871.

The Eye of the Mole.—Dr. R. J. Lee shows that, though the fœtal mole has an optic nerve, foramen, &c., both are completely atrophied or absent in the adult.—*Lancet*, July 9th, 1870, p. 55.

Anterior Epithelium of the Cornea.—The lowest layer of the anterior epithelium of the mammalian cornea consisting of cylindrical cells, contains in scanty distribution peculiar cells, whose chief distinction from those around them depends on the nature of their nuclei, which show remarkably granular oval corpuscles, whose intercellular layer mostly is with difficulty to be shown. In fine diagonal sections of the cornea through the epithelium, these corpuscles lie at an almost uniform little distance from the anterior elastic layer. Quite freshly examined, they appear more pale, by help of reagents as ellipsoid forms, which contain numerous heterogeneously grouped and arranged longish granules in a clear ground substance. Neither with the wandering cells of the corneal epithelium nor with the always double contoured nuclei of the neighbouring epithelial cells are these corpuscles to be confounded; the origin and signification of which is as yet entirely obscure.

From successive horizontal sections it is evident that the reproduction of the corneal epithelium does not proceed in the inferior, but essentially in the middle layers by division of nuclei. According to which one might have therefore to distinguish an inferior constant cylindrical-epithelium, and a superior constantly renovated division of the epithelium, consisting of flat cells.—*Centralblatt für die Medicinischen Wissenschaften*, May 28th, 1870, p. 383.

Intimate Structure of the Vitreous in the Human Eye and in that of the Vertebrata generally.—G. V. Ciaccio arrives at directly contrary results to those hitherto published, asserting that the minute structure of the vitreous in fœtal life and in adults is absolutely the same; that the whole mass of it consists of a very transparent sticky homogeneous matter, and also of an extremely fine inextricable net interspersed in the same of extraordinarily numerous fine fibres felted together, which are intimately attached to the inner surface of the hyaloid. The fibrous network is especially evident when the vitreous has been treated with alcohol and chromic acid, but in the perfectly fresh condition traces of it may be discovered.

In the substance of the vitreous the author could never find the cells spoken of by preceding observers. On the other hand, in the fœtus as well as in adults, the whole inner surface of the hyaloid (as is easily shown by treating the vitreous prepared with the hyaloid uninjured, with carmine, fuchsine or osmic acid of half p. c.) is, as it were, thickly sown with roundish granulated cells (cellulæ subhyaloidæ), to which the author ascribes great importance both for the first formation of the vitreous and for its nutrition and regeneration.

In repeated chemical analyses of the vitreous the author always found water, NaCl, sometimes also NH_4Cl , a certain albuminous body, which, to Millon's reagent — NO_5 , and NH_4O , behaved like the white of a hen's egg, from which, however, it is distinguished by the low power of coagulation with heat, by alcohol, NO_5 , and tannic acid, and in small quantity (in the fœtus always more abundantly than in adults) a fat extractible by ether, but never the mucin announced by Virchow and others.—*Moleschott's Unters. z. Naturlehre*, X, 583—9.

Dioptries.—Dr. Dudgeon wishing to see distinctly in diving under water, has invented a biconcave *air* lens, or one made up of two sections of a hollow sphere of glass (two and a half inches diameter), the convexities towards each other, and their edges united by a water-tight metal rim. This is to replace the loss of the refractive power of the aqueous humour ("anterior lens"), and instead of its equivalent (+ 2) glass lens, which had to be adjusted to the eyes each time on going below the surface of the water. The *air* lens offers no obstruction to vision above water. It seemed to the author "the perfection of simplicity to compel the water which had taken away the vision to restore it by a new arrangement of itself . . . The media, which in the normal condition caused perfect vision, air and water, were again employed, but in an inverted manner."

Dr. Dudgeon considers that the reason why authors differ so much as to the actual convexity of the anterior surface of the lens is that they have not observed that it is not the segment of a sphere, but that its external portion has a smaller radius of curvature than its centre (11.5 and 14 mm.). The latter alone concerns us.

Donders and others have overestimated the increase of curvature of the anterior surface of the lens in accommodation for near objects. The eye, after extraction of the (cataractous) lens, is compared to a spherical water lens, composed of the united aqueous and vitreous humours, with identical densities.—*Notes on the Dioptries of Vision* (pamphlet).

Phænophthalmotropia.—Under this designation Donders describes an apparatus, by means of which the movements of the eye, according to Listing's laws, may be represented. It consists essentially of a globe, which may be turned about within three rings around three axes standing perpendicularly on one another. By

turning within of the outermost ring every desired inclination can be imparted to the originally horizontal turning axis which was directed from the lateral to the median side, in which case it remains always in the plane perpendicular to the sight line. It is known that according to Listing's laws the eye turns around an axis, which stands perpendicular upon the first and second position of the sight line, which condition therefore is fulfilled in the apparatus. Also the inclination of the representations of horizontal and vertical lines and the solution of Helmholtz of the movements of the eye (raising and lowering, turning laterally of the sight line, rotation) may be demonstrated by the model, of which in the original a full description is set forth.—*Centralblatt für die Medicinischen Wissenschaften*, July 16th, 1870, p. 510.

Test Types.—Dr. B. Joy Jeffries, judging by his own vision, doubts if our tests are sufficiently nice—if the power of reading Snellen's No. XX, at 20 ft., is not considerably below the average. The doctor, *e.g.*, can read No. VIII of Snellen's types at the same distance, "when not feeling below par in general sensation of health." Of some recruits he has examined, he says:—"The same men who the day after a spree could not read my Snellen XL or L, would come back the day following and read XX fluently."—*Transactions of the American Ophthalmological Society*, 1869, p. 43.

Accommodation.—Brought about in the human eye by the ciliary muscle. Mr. Hulke infers that the normal condition of the lens at rest is to be compressed and flattened, especially on its anterior surface, by the action of its suspensory ligament, and that the ciliary muscle, by drawing forward the choroid and processes, and slackening the ligament, allows the lens to assume a greater degree of anterior convexity.—*Lancet*, July 9th, 1870, p. 62.

Traumatic Paralysis of Accommodation.—Dr. G. C. Harlan reports three cases of this nature, two of them uncomplicated, a rare result of injuries. Dimness of vision and a widely dilated pupil were the leading symptoms *primâ facie*. Calabar bean was applied locally, and recovery was complete and speedy.—*Amer. Journ. Med. Science*, Jan., 1871, p. 139.

Paralysis of the Ciliary Muscle from Diphtheria.—Three cases of Mr. Hutchinson's are reported. The patients were children, and unable to read, and, unless there were also hypermetropia present, as in other cases of the kind, distant objects were well seen without a convex glass, such as is needed for reading when the muscle of accommodation is paralysed, without hypermetropia. "In a month or six weeks the sight is again perfect."—*Lancet*, Jan. 7th, 1871, p. 13.

Epileptic Pupil.—Dr. Clouston, as to the various states of the pupil in insanity, of which he considers a permanent contraction to be the most unfavorable prognostic, makes the following remark, "1

have observed a condition of the pupil in an epileptic which I have not seen described. The patient is a youth of seventeen, who has taken epileptic fits from birth. After each fit, just when the tonic spasm has ceased, if the eye is kept open and the pupil watched, there occurs what appears to me to be an epileptic convulsion of the iris. It dilates to its utmost extent, and then contracts rhythmically for over two minutes at least. The movements are, of course, slower than the jerking convulsions of the voluntary muscles, but appear to me to be of the same character. From what I have observed in other cases too, I am inclined to think that the hitherto unexplained movements of the iris during and after an epileptic fit, are in reality of the nature of the spasmodic movements of the voluntary muscles."—*Practitioner*, July, 1871, p. 21.

Demonstration Ophthalmoscope.—MM. Wecker and G. Roger presented to the Paris Académie des Sciences, a prismatic objective, with which two observers can explore together the interior of the eye.—*Archives Générales de Médecine*, June, 1870, p. 756.

Ophthalmoscopic Demonstration.—Dr. C. M. Gariel, in his recent publication, Paris, 1869, on the ophthalmoscope, has shown the possibility, theoretically, of getting objective images thrown on a screen, and thus, by a modification, to make them visible, at one and the same time, to a number of persons.—*Gazette Hebdomadaire*, Dec. 17th, 1869, p. 816.

Ophthalmoscopic Signs of Death.—M. Poncet, of Strasbourg, has been investigating this point in man and animals. He has invented (and figured) a new ophthalmoscope for this purpose, which requires the eye to be observed in a dark chamber. In four to five hours, or less, the cornea is often too opaque for light to be transmitted, if the eyelids have not been closed. The cornea should be wetted with a drop of water to restore its transparency. Immediately after death the arteries generally disappear, and the veins are only shown by small, indistinct, irregular clots here and there. The choroidal plexuses are replaced by whitish or slightly rosy streaks in the pigment. The optic disc is yellowish white, in a fundus generally pale and doubtful in its aspect, without vascularisation but for the clots mentioned. The fact of death having occurred, or of the exact moment when it has occurred, may thus be determined in any doubtful cases.—*Archives Générales de Médecine*, April, 1870, pp. 408-24.

Ophthalmoscopic Signs of Spinal Disease.—Dr. T. C. Allbutt, in reviewing the facts, as they are known at the present time, adds to them his own observations. He says, in eight of thirty well-marked cases of spinal injury, secondary disturbance within the eye was found. These changes do not become established in the cases which run a short course, but they slowly supervene in the course of weeks or months in more chronic cases. In spinal diseases arising from injury, the higher the seat of the injury the sooner are these changes

in the eye. The (ophthalmoscopic) morbid appearances he arranges in two heads, the second including the traumatic cases: 1. Simple or primary atrophy of the optic nerve, sometimes accompanied, at first, by that slight hyperemia and inactive proliferation which make up the state I have called chronic neuritis. This sort of change he has never found as a result of spinal injuries, but he has often met with it in chronic degeneration of the cord and in locomotor ataxy. 2. A somewhat characteristic hyperemic change not seen in chronic degeneration, or in locomotor ataxy, but in cases of injury of the spine only. The retinal arteries do not dilate, but become indistinguishable, while the veins begin to swell and become somewhat dark and tortuous. The disc then becomes uniformly reddened, and its borders are low, the redness or pinkness commencing with increased firm vascularity at the inner border, which thence invades the white centre and the rest, so that the disc is obscured, or its situation known only by the convergence of the vessels. In many cases, rather than redness, he has observed a delicate pink—pink which sometimes passes into a daffodil colour. It ends in resolution rather than in atrophy. Sometimes the sight suffers a good deal in these cases, sometimes but little or scarcely at all. He has not seen true optic neuritis, with active proliferation, as a sequel of spinal disease. As to the processes connecting the changes in the spine and in the eye, the author, in the first place, objects to Prof. Wharton Jones's recent explanations of injury to the cord causing the sympathetic nerve or its origins to be involved, and so are, consequently, the blood-vessels, and nutritional changes are superinduced in the eye. He also objects that he has not observed, ophthalmoscopically, the dilatations of the arteries at and above the disc, to which Mr. Jones alludes. Can a palsied sympathetic be the ruin of the optic disc, when its effects are unseen in the pupil, unseen in the conjunctiva, unseen in the ear and cheek? Again, he says, it is a matter of verified observation, in numerous cases in which there have been most obvious signs of a palsied sympathetic in the ear, face, and outer eye, that in these very cases the back of the eye has been found unchanged. He is led towards the conjecture that hyperemia of the back of the eye, following injury to the spine, is probably dependent upon a greater or less extension of meningeal irritation up to the base of the brain. Spinal meningitis does creep up to the encephalon, and if this ascending meningitis be the correct explanation, it accords with his observation that, in general, the higher the injury to the spine the sooner the affection of the eye. Atrophy of the discs is seen, not in injuries of the spine, but in slow degeneration of the cord, in cases, that is, where meningitis is usually absent or inactive, and it is seen most frequently by far in that degeneration of the cord called sclerosis of the posterior columns—of that part of the cord which tends to travel towards the encephalon, and not towards the periphery—up to the cerebellum.—*Lancet*, Jan. 15th, 1870, p. 76.

The Ophthalmoscope in the Treatment of Epilepsy.—Dr. Reuben Vance, of New York, distinguishes two forms of the disease, diagnosed

by the state of the circulation as seen in the retina;—the hyperæmic to be treated with bromide of potassium and depletion—and the anæmic with quinine, iron, and strychnia. In the latter class, the brain being anæmic during sleep, becomes a predisposing cause of the fits. In the congestive form they would be more likely to take place during the day.—*Lancet*, March 11th, 1871, p. 356.

The Ophthalmoscope in Cerebral Disease.—Dr. Hughlings Jackson would have the instrument always used in cases of doubtful nature, perhaps the so-called “bilious fever,” headache, vomiting, &c.; and if there is no renal disease, and the neuritis is double, it is strong evidence of some kind of coarse disease of the brain. *There may be neuritic changes, although the patient can read the smallest type.* The establishment of this and similar facts will enable physicians to foretell probable failure of vision in some cases.—*Medical Times and Gazette*, June 3rd, 1871, p. 627.

Congestion Papilla.—Mr. Swanzy reports a case of this kind in which he diagnosed, and there was found, after death, a tumour of the brain. The patient had constant headache, frequent vomiting, great drowsiness, and loss of memory. The pupils were dilated widely. One eye had slight perception only of light. He was ill four months. The tumefaction of the optic disc was very prominent, but did not extend far into the surrounding retina. The retinal veins were very much engorged and tortuous, disappearing where they turned round the edge of the tumid part. The extreme prominence of the papilla is depicted in a woodcut showing the eye, in two parts, cut open. The ophthalmoscopic appearances are given as a chromolithographic illustration. The author concludes by contrasting this state with that in neuritis descendens; the pathological changes in congestion papilla ceasing at the lamina cribrosa.—*Dublin Quarterly Journal*, February 1st, 1871, pp. 226—7.

Meningitis and Pseudo-Meningitis.—M. Bouchut characterises the latter as a morbid state of functional disturbance of the brain and meninges; as an acute febrile temporary state of disease, shown by fever, irregularity of pulse, vomitings, constipation, and pains in the head, brought on by reflex congestion of the membranes. It is particularly an infantile complaint. Not so only. The author goes on to say that by *cerebroscopy* one may discover in the eye unmistakable evidence of meningeal congestion. He refers to his former researches in this respect with the ophthalmoscope, from which he would deduce the following conclusions:

Congestion of the papilla indicates congestion of the meninges and of the brain.

Hyperæmia of the retinal veins, distended and filled by stasis or by clots, indicates repletion of the sinuses and thromboses of the meningeal veins.

Retinal hæmorrhages indicate venous compression.

Aneurisms of the retinal arteries indicate a similar lesion of the smaller cerebral arteries.

Papillary œdema indicates thromboses of the sinuses or of the cerebral veins.

Dotted choroidal atrophy indicates an incomplete general atrophy of the brain.

Atrophy of the optic nerve indicates sclerosis of the optic layers, and sometimes of the antero-lateral columns of the spinal marrow.

Cellular and granular exudations of the optic nerve indicate sclerosis of the nerve and its roots.

Tubercles of the retina and choroid indicate tuberculosis of the meninges or general tuberculisation, &c.

In fine, in most of the cases in which the functions of the cerebro-spinal system are disturbed, one may see something analogous in the eye.

In pseudo-meningitis, in face of more or less serious appearances or meningitis, embarrassing symptoms seeming to indicate the outset of this disease, one finds in the fundus oculi a congestion of the optic nerve without choroidal colouration or distension of the retinal veins, *i. e.*, a lesion signifying cerebral and meningeal congestion without acute inflammation of the meninges; in which case there would have been either peripapillary œdema or distension, varicosity, stasis, and thrombosis of the veins of the retina. The only difference between the true and false meningitis is a question of degree. In both, meningeal congestion is shown by papillary hyperæmia, but in true meningitis the ophthalmoscope shows a higher degree, the circulatory difficulty of the brain is indicated by papillary œdema, phlebotretinal stases and thromboses and retinal hæmorrhages or tubercles of the choroid, the clots of the sinuses, the ventricular effusions, and the tubercles of the meninges. The progress of the accidents only enables us to distinguish the true from the false meningitis. The pseudo-meningitis does not terminate in convulsions and paralysis. The author distinguishes five or six varieties of this disease.—*L'Union Médicale*, Nov. 11th, Dec. 4th—25th, 1869.

Uræmic Amaurosis.—Dr. Argyll Robertson distinguishes this from the comparatively common cases of albuminuric retinitis by saying, "Where the dimness of sight is due to uræmia, the amaurosis occurs suddenly, reaching its height in several hours—in some cases even in a few minutes. And the blindness is not unfrequently complete. Resolution occurs equally rapidly. In the other form the sight may also become pretty rapidly impaired at the commencement, but thereafter progresses more gradually, and after increasing for several days, remains stationary for a longer period. Very seldom does absolute blindness ensue. Resolution, too, when it does occur, proceeds very slowly." The author points out that not unfrequently, "from the insidious progress of the disease," the special ophthalmoscopic signs lead to detection of the serious general disorder.—*Edinburgh Medical Journal*, January, 1871, pp. 615—23.

Dr. Clifford Allbutt quotes an illustrative case. First of all the patient had had albuminuric retinitis, which had damaged his retina in certain districts only. Subsequently his sight became very

defective. This dimness of vision varied a good deal ; at one visit he could scarcely distinguish objects at all, or count fingers ; while at the next he wanted only clear vision of details. There were some symptoms of uræmia existing, such as drowsiness and occasional vomiting.—*Lancet*, June 3rd, 1871, p. 746.

Glycosuric Amblyopia.—Dr. Fitzgerald related to the Dublin Pathological Society a case of dimness of vision in diabetes. There was no cataract, no hæmorrhages, only atrophy of the optic nerve and retina, and (the choroidal vessels and pigment islands were very well seen) the epithelial layer of the choroid implicated, “but this is only natural when we consider its very close relation with the external layers of the retina.” The opposite eye had been lost by an injury seven years previously (the lens dislocated).—*Dublin Quarterly Journal*, August 1st, 1870, p. 226.

Bloodletting in some Eye Diseases.—Mr. Hamilton, of Liverpool, protests against the almost entire disuse of local bloodletting. He employs leeches and cupping, and relates three cases, characterised by more or less evident hyperemia of the optic nerves and dilated pupils, corroborative of his views, of which also he gives a rationale.—*The Practitioner*, June, 1870, pp. 340-8.

Squint Downwards.—Surgeon Partridge, of Bombay, reports this case:—Captain E—, æt. 45, generally used only the right eye, and had acquired a habit of half closing the left at the time. Both being open, the left eye was turned directly downwards, or downwards and very slightly inwards. There was no paralysis. “Both eyes being uncovered, and he being told to look with the left eye only, the right eye was turned somewhat upwards.” The left inferior rectus was divided subconjunctivally and the squint perfectly cured. In the left eye ($V=\frac{1}{4}$) the patient had mixed astigmatism. Glasses for the two eyes were ordered.—*Medical Times and Gazette*, March 4th, 1871, p. 243.

Insufficiency of the Internal Recti.—Dr. Swanzy says in muscular asthenopia (as distinguished from the accommodation) there are pains and sensations of tension, more especially, perhaps, in the brow. In accommodation asthenopia, “if work be proceeded with, the objects soon become indistinct ; in muscular asthenopia this is also the case ; but here, if the patient be reading, the indistinctness consists more in a shifting of the words and lines over each other than in a general cloudiness of them. In both forms a shorter or longer rest enables work to be proceeded with again ; but in the muscular form the benefit derived is less striking ; in it the rest must be of longer duration, and the periods of work shorter. Many persons suffering from muscular asthenopia learn to cover one eye with the hand during close work, by which means they succeed in evading the troublesome symptoms.” As a certain test, he recom-

mends the "equilibrium test," making use of the physiological fact that a prism stronger than 1° or 2° , held with its base upwards or downwards before one eye, produces inconquerable diplopia, such as, in other words, few can by their superior or inferior rectus overcome. Then a dot on a piece of paper will appear as two, the one not only above the other, but to one side; for no muscular effort can result in single vision, and so the eye passively deviates outwards. It is accurately measured and recorded, and the details more or less of the operative procedure (tenotomy of the external rectus) are by this to be accurately regulated, according to an empirical scale of Von Gräfe's. The gymnastic treatment is given up.—*Dublin Quarterly Journal*, Aug. 1st, 1870, pp. 35-19.

Myopia in the Army.—M. Giraud Teulon says, "Myopia is a cause of exemption in the French but not in the German army, in which it could be so much more often successfully pleaded as appertaining especially to the more studious nation." In the French army the number of exemptions on the ground of myopia does not exceed 4 or 5 per 1000 in the country districts, while in the town districts it is much larger. In the Paris Polytechnic School also, at one of the recent inspections for admission, myopia was found to be as high as 35 per cent. The wearing spectacles may well be set against the advantage of having soldiers who are well instructed, and able to find their way in an unknown country by means of the constellations." To obviate simulation of myopia in a recruit he would always adopt the ophthalmoscopic test. Medical examiners want instruction. Myopic soldiers should not be put to services calculated to aggravate the disease.—*Med. Times and Gaz.*, Aug. 27th, 1870, p. 246.

Blepharitis.—Mr. Carter very truly remarks that neglect on the part of the patient, and want of painstaking on the part of the surgeon, are the causes of its ill results. At the outset he uses a warm alkaline lotion—five grains of bicarbonate of soda to an ounce of water.—*St. George's Hospital Reports*, 1871, p. 351.

Purulent Ophthalmia.—Mr. Power considers it probable that this ophthalmia, *i. e.* an ophthalmia with a purulent discharge, although it goes on to a rapidly fatal result, may be merely a catarrhal ophthalmia in a weak or cachectic subject.—*Ibid.*, 1871, p. 322.

Sympathetic Ophthalmia.—Mr. Power considers that this never occurs unless the injured eye is anautotic, *i. e.*, as he afterwards tells us, completely blind.—*Ibid.*, p. 325.

Cataract: Early Operating.—When both eyes are affected by cataract, and this is progressing equally and slowly, Mr. Carter would hasten the maturation of the cataract in one eye by opening the capsule with a needle—a practice much followed, we believe, of late by Mr. Bader, of Guy's. The author would even do this, under certain extraordinary circumstances, if the non-cataractous eye were blind.—*The Practitioner* March, 1871, pp. 165-6.

He contrasts the advantages of this artificial maturation with the method of extraction of the immature cataract in its capsule. "In all cases in which trouble is to be anticipated from the adhesive character of the still transparent cortex, the surgeon has the additional resource of extracting the lenticular system in its entirety, the lens in its unbroken capsule. But a transparent cortex generally implies a condition of rather firm connection between the hyaloid and the posterior capsule, and there is then constant liability to loss of vitreous prior to the escape of the lens. When this happens, or when the lens resists moderate pressure, it becomes necessary to use traction instruments; and the operation has then been followed, in some instances, by inflammation and cell-proliferation in the vitreous itself, causing turbidity that only very slowly disappears. Moreover, the extraction in the unbroken capsule requires a very large iridectomy, likely to be productive of dazzling and imperfect vision; and it also requires a rather large external section, which increases the risk to the cornea. On the whole, therefore, the dangers of this method are probably greater than those of puncture as a preliminary to extraction, and its advantages in respect of saving time and of perfection of vision are only obtained when the course of events is altogether without accident or complication."—*St. George's Hospital Reports*, 1871, p. 345.

Corelysis and Paracentesis for results of Iritis.—Mr. Carter suggests that Streatfeild's spatula-hook should be made to *cut* in the notch, for which purpose the latter should be somewhat larger than he made it. Mr. Carter fears irido-dialysis, and he further suggests that the spatula-hook be made bent on the flat for its use on the *nasal* side of the eye. When, in iritis, the tension is increased, and only recently, Mr. Carter does paracentesis and evacuates the aqueous humour, "by opening the little incision by a fine probe, or by the beak of Weber's lachrymal knife. In most cases it will be desirable to introduce the probe two or three times a day for the first few days, and thus to establish a drain of aqueous humour, which will, in fact, be a sort of indirect depletion of the vessels of the eye.' (In a case of the intractable creeping ulcer of the cornea Mr. Carter incised it, and every day, for a week or ten days, reopened the incision along its whole length by the beak of a Weber's knife.)—*Ibid.*, 1871, pp. 356-7-9.

Melanoma of the Cornea.—Supplementarily to his experimental investigations on the formation of pigment in extravasation, Th. Langhans sought also to infer the pigmentation of melanotic tumours from the transformation of cells containing blood-corpuscles. In the case of melanoma of the cornea investigated by him, cells containing blood-corpuscles could not, of course, be any longer shown, as they had already many months been lying in spirit, but the pigment was collectively enclosed in cells, and the larger pigment-granules had yet, in part, clearly the form and size of the normal discoid or the altered globular blood-corpuscles; the colour of the

pigment was dark brown. By other observers cells containing blood-corpuscles have been found repeatedly near hæmorrhages in melanotic tumours. When, as in most melanotic tumours, no hæmorrhages have been found, penetration of red blood-corpuscles through the coats of the vessel may be assumed, and this action would be very much favoured by the capacious and thin-walled vessels of the melanoma.

The tumour in question, which was interesting in other particulars, was situated on the cornea, with a narrow base, fungus-like. Its surface was smooth, with little pigment; the deeper parts were extensively coloured of a dark brown by porous-like layers of distant vessels. It was placed upon a fibrous pannus-like mass, which grew from the conjunctiva, covering the anterior surface of the cornea, but without affecting its substance. On the other hand, a brown colouring extended from the tumour into the upper layers of the cornea, even beyond the tumour.

The coloured parts consisted of epithelial-like cells, which, without intercellular substance, were lying loosely together, and were partly pigmented, partly pigmentless. The cells were lying in round alveoli, the reticular spaces between which were occupied by vessels and closely compacted cells, which, for the most part, were more strongly pigmented than the cells of the alveoli themselves. In the parts non-pigmented the cells were much smaller, round, elongated, or spindle-shaped, closely compacted in a finely-granular or striped ground substance; the vessels were here also much finer. —*Centralblatt für die Medicinischen Wissenschaften*, May 14th, 1870, pp. 346-7.

Calabar Bean in Suppuration of the Cornea.—M. Galezowski says the contractile action exerted by the bean on the vessels of the cornea opposes their dilatation and congestion, and singularly aids the cicatrization of wounds. Belladonna, which produces opposite effects, should, he thinks, be discarded in these affections.—*Annuaire de Thérapeutique*, 1870.

False Colouring and Indefinition of Objects by Santonin.—Walter G. Smith, M.B., says, "On the 30th of August, a bright clear day, I took five grains of pure white santonin, half an hour after luncheon. While engaged in reading, some three hours after, I became gradually conscious of a yellowish tint on the paper, and fancied that there must be a yellow haze in the air. My own hands and the complexions of others appeared of a sallow, unhealthy colour, and the evening sky, which was really of a pale lavender tint, seemed to be of a decided light green. Vision was not perfectly distinct for some hours, and was accompanied by a certain vagueness of definition. This effect was followed each time after a dose of four or five grains of santonin, whether of the white or yellow modification."—*Dublin Quarterly Journal*, November 1st, 1870, p. 266.

Microphthalmos.—Mr. Wilson has exhibited to the Dublin Pathological Society an eye which he had extirpated. There had been a

metallic reflection from the fundus, but there was found to be no intraocular growth. The sclerotic was extremely thin, and the choroid almost pigmentless; hence the reflection, in the opinion of the surgeon. A small tumour (fibrous, and presenting several cyst-like cavities) was found attached to the posterior surface of the sclerotic and to the sheath of the optic nerve. The measurements approached those of most animals, the lateral diameter (S''') being greater than the antero-posterior ($7\frac{1}{4}'''$). A coloboma iridis "enabled us to fix the period of gestation in which this arrest of development occurred."—*Ibid.*, Aug. 1st, 1870, p. 214.

REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By ROBERT HUNTER SEMPLE, M.D.,

Member of the Royal College of Physicians, Physician to the Bloomsbury Dispensary, London.

Santonine as a Cause of Urticaria. By Dr. E. H. SIEVEKING.—Dr. Sieveking relates a case in which he prescribed three grains of santonine with five of sugar, the patient being a child of four years old, and very soon after this dose was taken vomiting occurred, accompanied by a severe rash, described to him as urticaria and covering the greater part of the body. As Dr. Sieveking thought that these symptoms might be due to some error in diet (which had, in fact, been committed), he did not attribute them to the santonine, and he ordered the dose to be repeated. But almost immediately after the medicine was taken an eruption appeared on the nose and spread over the whole body, attended with enormous swelling. This rash, however, soon disappeared after the use of a warm bath, and, although no vermifuge effect was noticed, the child's health was improved. As Dr. Sieveking thought that the santonine might be impure, this supposition was removed by an analysis of the specimen, and the effects produced upon the patient must therefore be classed among the category of those observed in certain constitutions by the use of copaiba, shrimps, mussels, &c.—*British Medical Journal*, February 18th, 1871.

On the Present Aspect of the Antiseptic System of Treatment in Surgery. By Professor LISTER.—A case lately occurred in Professor Lister's practice of a young man who had sustained a fracture of the ulna and a dislocation of the upper end of the radius. The case had at first been mistaken, and when it came under Professor Lister's observation the dislocation was unreduced and the broken ends of the ulna had united at an obtuse angle, so that the limb was practically almost useless. It therefore became necessary to break the ulna again, with a view to make the forearm straight and to reduce the dislocation. The steps taken for this object were ma-

terially promoted by the employment of the antiseptic treatment. The skin was first washed with a watery solution of carbolic acid, in order, Mr. Lister observes, to destroy all putrefactive particles in the epidermis and hair-follicles, and then an incision was made while an assistant threw over the part a cloud of spray of carbolic acid by means of Richardson's apparatus. The newly united bone was broken through by means of a pair of strong bone-pliers, smeared with an oily solution of the carbolic acid, and as it was impossible to reduce the dislocation of the head of the radius, this part was removed at the neck of the bone by means of the pliers, again smeared with oil, and the parts concerned in the operation being kept bathed in a cloud of spray. A folded cloth dipped in a solution of the acid was first laid upon the wound, and the whole of the limb was enveloped in lac plaster, the cloth having been previously removed. The dressings were changed the next day, and again two days afterwards, the coagulated blood being removed and the carbolic acid being again applied, but now the dressings were left untouched for four days, at the end of which time there was no pus at either of the wounds and healing was going on under a scab. "In other words," says Mr. Lister, "putrefaction being excluded by means of an efficient antiseptic guard, while the exposed tissues were protected from the action of the antiseptic salt by the interposition of a layer of unstimulating material, the disturbing influence of external agency was avoided, and we attained very closely to the condition of a subcutaneous injury." Mr. Lister attaches great importance to the employment of the spray in operations like that which he here records, for as he attributes suppuration to the presence of putrefactive organisms in a wound, he considers that the spray, by penetrating into every part of the exposed surface, effectually prevents the development of any such germs, some of which might otherwise escape the action of the antiseptic lotion and subsequently propagate their kind.—*British Medical Journal*, January, 14th, 1871.

On the Mode of Action and Use of Digitalis. Being the Hastings Prize Essay for 1870. By Dr. J. MILNER FOTHERGILL.—In this elaborate essay Dr. Fothergill discusses in succession all the chemical, physiological, and therapeutic history of digitalis. After a sketch of the effects produced by infusions of this plant upon other plants and the lower animals, he next considers its manner of action on the animal body and the clinical results following its employment in medicinal doses. With regard to its manner of action two theories have been proposed,—one, that of Traube, that it causes paralysis of the fibres of the pneumogastric nerve; the other, which has been adopted by several foreign and British authorities, that it acts as a stimulus to the cardiac ganglia and thus produces increased muscular contraction. The latter view is unhesitatingly adopted by Dr. Fothergill as being most consistent with experiment and with clinical observation. In fact, it is proved that the heart of a frog, when taken out of the body, and dipped into a solution of digitalin, was arrested in its rhythmical movements owing to contraction of the

muscular fibres, and the application of an infusion of digitalis to a frog's foot produced contraction of the vessels of the web. In both these cases it is obvious that the pneumogastric nerve could have no part in the phenomena observed, while the results are explained on the supposition of a stimulant acting upon the vaso-motor or sympathetic fibres. Clinical observations point in the same direction, for the action of digitalis in therapeutic doses is to lower the pulse by promoting contraction of the ventricles of the heart and of the muscular fibres of the arteries. The action of digitalis on the heart and arterial system is that of a stimulant to the sympathetic nerve fibres, whereby contraction is produced, and consequent diminution of the pulse.

Digitalis was formerly regarded as a sedative because it relieves palpitation and irregularity, but these effects are produced, not by its lowering excessive action, but by its stimulating the muscular fibres to contract. Palpitation is not a sign of over-action, but of weakness of the heart, and hypertrophy is, without exception, only a compensating growth to overcome some obstacle. Palpitation is an evidence of over-taxation, whether it occurs in hypertrophy or dilatation, and the administration of digitalis is followed by a quieter action of the heart, because under the use of this drug the beats of the organ are brought nearer to the normal condition. It is important to remember that when the heart is acting regularly we are quite unconscious of its movements, and when we begin to be conscious of them there is the proof of its action being faulty. The diseases of the heart for which digitalis may be administered are very various, and indeed opposite in their pathological origin, and Dr. Fothergill examines these conditions *seriatim* in order to show in what way the drug proves beneficial in each case. In hypertrophy the action of digitalis is perhaps most readily observed; for as hypertrophy is essentially a compensatory condition, the balance between the blood to be driven and the power to drive it is being already re-established by the efforts of nature. Hence small doses of digitalis produce marked results in hypertrophy because nature is co-operating with the drug, but much larger doses are required in dilatation because there is no compensating hypertrophy to restore the disturbed balance, and hence a greater stimulus to contraction is necessary to produce the desired results. In valvular disease no effect is produced by digitalis except so far as the drug acts on the muscular walls of the heart, but the alteration of the muscular walls is compensatory in this disease, for, as the blood has to pass through a narrower orifice, a greater power is required to propel it, and the thickening of the walls provides this power. The administration of digitalis assists the natural attempts made to overcome the resistance offered by the obstructed valves. Dr Fothergill ingeniously shows that while digitalis is efficient, for the reason first mentioned, in mitral obstruction, it is also beneficial in mitral regurgitation, for in the latter condition the *right* ventricle requires to be thickened and strengthened in order to prevent its over-distension from the accumulation of blood *a tergo*, and on the other hand the *left* ventricle, when digitalis is administered, is made to contract in such

a manner as to prevent too much blood from being poured into it under the greater pressure from behind. In aortic obstruction digitalis is useful because it increases the driving power of the heart, and co-operates with the hypertrophy of the left ventricle, which is the compensatory condition in this disease. In aortic regurgitation, however, it is questionable whether any treatment at all is beneficial, and digitalis is rarely indicated. In the earlier stages it is contra-indicated by the hypertrophy and sustained contraction of the left ventricle, and digitalis might do more harm than good by driving the ventricle into a state of contraction which might never be relaxed, or by bringing the heart to a standstill in systole. In tricuspid regurgitation no remedial means are much to be relied upon, and the administration of digitalis is apparently of no benefit whatever. On the whole, in all valvular affections, digitalis is useful or useless entirely in proportion to its power of producing conservative compensatory changes in the muscular walls; "in disease of the left side, by following the indications thrown out by spontaneous accessory growth, we may be of the greatest service in either aiding the production of the compensatory change, or assisting in its continuation when beginning to fail." The degeneration of the walls of the heart was once thought to be a condition contra-indicating the use of digitalis, when the drug was considered as a cardiac sedative, but now that it is regarded in an opposite light, it may be administered with confidence. In degeneration of the muscular fibrillæ no action can affect the structures converted into fat-globules, but digitalis, by acting on the sound fibrillæ, may produce relief of symptoms, and, even according to Dr. Fothergill, may aid in somewhat improving the structural condition, though this improvement will depend to a great extent on the pathological cause of the degeneration. As digitalis must now be regarded as a cardiac stimulant, increasing the contractile power of the muscular fibres, it follows that many conditions of the heart which were formerly considered to contra-indicate its use are at present regarded as likely to be benefited by it, and Dr. Fothergill passes in review a number of cardiac affections, both structural and functional, in which digitalis may be beneficially employed.

In several diseases, also, not of cardiac origin, digitalis has been successfully administered, especially in delirium tremens, in which it has been given in half-ounce doses, frequently repeated; and it has been recommended, though on doubtful evidence, as a remedy in some nervous affections, as in epilepsy and hemicrania. With regard to the mode of administering digitalis, Dr. Fothergill considers the tincture as being usually the most convenient form, but in the treatment of chronic conditions, when it is necessary to keep up the administration for a lengthened period, he prefers the powdered leaves.—*The Hastings Prize Essay, 1870.*

On the Corrective Influence of Bromide of Potassium on Opium.
By Dr. J. M. Da Costa, of Pennsylvania.—Dr. Da Costa has met with several cases in which he was able to counteract the unpleasant

effects of opium by the administration of bromide of potassium. His first case was that of a lady who suffered from abdominal pain of a most severe character, but who was unable to take opium for its relief in consequence of the distressing symptoms caused by that drug. The pain, indeed, was relieved, but there was itching and tingling all over the body, numbness, sense of sinking and faintness, and semi-unconsciousness. But by administering the bromide of potassium all these unpleasant consequences were prevented, and she was able to take the opiate preparations with comfort. The beneficial effect of the bromide, however, was not equally observed with all the forms of opium, for it had little influence on morphia, and not much on codeia. Still, it had some influence even upon these alkaloids. Another case was that of a lady subject to attacks of diarrhœa, in whom all opiates, even paregoric, produced faintness, headache and nausea, but by taking twenty grain doses of the bromide before taking the opium these results were averted. Two other cases of a similar nature are given, in which the bromide acted equally well. Dr. Da Costa states that the bromide does not destroy either the anodyne or the hypnotic effects of the opiate, but, on the contrary, it heightens both, and it has seemed to him that it acts best when given some hours before the opium, and that forty to sixty grains prove sufficient. When morphia is used hypodermically, it is then necessary to give the bromide some time in advance, and it may require to be given in larger doses. Dr. Da Costa admits that, although the corrective influence of the bromide on opium holds good as a general truth, exceptions to the rule are occasionally met with. —*American Journal of the Medical Sciences*, April, 1871.

On the Treatment of Psoriasis by Balsam of Copaiba. By Dr. H. S. PURDON, of Belfast.—The balsam of copaiba was found to possess special virtues in the treatment of psoriasis by the accidental circumstance of a patient having been admitted into the Hospital of St. Denis, in Paris, suffering at the same time from gonorrhœa and psoriasis, when, the copaiba being administered for the cure of one disease, it was found that both were cured at the same time. M. Hardy therefore introduced copaiba into the list of cutaneous therapeutical remedies. Dr. Purdon states that he has had lately under his care an unusually large number of cases of psoriasis at the Belfast Hospital, and that he has met with great success from the administration of large doses of balsam of copaiba, given with a little liquor potassæ, mucilage, and water. He also states that he has been able to discharge his patients sooner by means of the balsam treatment than by any other, and, moreover, that none of them have yet had a relapse, but still he admits that it is too early to speak positively on this point at present. He endeavours to account for the beneficial effects of the copaiba in psoriasis by supposing that this balsam acts as an irritant of the solar plexus, which through the medium of the vaso-motor nerves then causes inflammation of the skin in the form of urticaria or erythema. He then quotes John Hunter in his well-known aphorism, that two inflammations cannot coexist, as one

of them destroys the other, and hence the development of urticaria, which is an inflammatory disease, neutralizes the other inflammatory disease, which is psoriasis. Whether this explanation be true or not, Dr. Purdon has found the plan successful, and he gives an account of one case under his care in which the copaiba treatment was causing the disappearance of the psoriasis, but chorea supervened, for which iron and arsenic were administered. He raises the question whether the same morbid cause gave rise to the psoriasis and the chorea, or whether the psoriasis was "transformed" into the chorea, according to one of the views of Trousseau. Dr. Purdon's patient recovered completely from both diseases.—*Dublin Quarterly Journal of Medical Science*, May, 1871.

On the Use of Hypodermic Injections, especially in Cholera and Dysentery. By Dr. T. J. GALLAHER, Pittsburg.—Dr. Gallaher gives in the first part of his paper a general sketch of the history of hypodermic injections, assigning the discovery of this important method of medication to Dr. Alexander Wood, of Edinburgh, but likewise giving a due meed of praise to Dr. Charles Hunter for the zeal and ability with which he pursued his investigations on the subject. These gentlemen differed in opinion as to the efficacy of local as compared to general hypodermic treatment; but it seems now generally admitted that there is no marked difference in the effects of a drug subcutaneously injected, whether it be introduced near to or at a distance from the affected part. Dr. Gallaher passes in review the different preparations which have been employed hypodermically, and the maladies in which they have respectively been found useful, as, for instance, morphia in painful affections generally, atropia in spasmodic and nervous diseases, quinia in ague and diseases of debility, Calabar bean in tetanus, &c. The dose of the drugs hypodermically injected is from one fourth to one half of that usually given by the mouth; and with respect to the proper place for inserting the remedy, Dr. Gallaher considers that those parts of the body which are supplied by numerous superficial veins should be avoided, and he recommends the arm, near the insertion of the deltoid muscle, as the most eligible situation. The injection of a powerful poison directly into a vein might be followed by dangerous and even fatal effects, and therefore the above precaution is necessary. Dr. Gallaher gives the particulars of some cases of cholera and dysentery successfully treated by himself by the hypodermic use of morphia, these diseases not having been previously treated, as he believes, by this method. The results in all the cases were very satisfactory.—*New York Medical Journal*, May, 1871.

On the Antiphlogistic Treatment of Children. By W. C. ROBERTS, M.D., Vice-President of the New York Academy of Medicine.—Dr. Roberts commences his paper by an inquiry into the question whether children require to be treated differently to adults, and he shows that the peculiarities of infant life influence very much the nature of children's diseases, and consequently the therapeutical

means required to treat them successfully. He also examines the great question as to the nature of inflammation, and although not pronouncing a dogmatic opinion on the subject, he considers that in all strongly developed acute inflammations there is an excess of fibrin and of colourless lymph-corpuscles in the blood, and that coincidentally there are the symptoms of fever, with a hard pulse instead of a soft one. In order to relieve the inflammatory condition he thinks that antiphlogistic remedies should be used, of which bleeding is the chief, and that catalytic agents, such as mercury and antimony; sedatives, as opium, veratria and aconite; and diaphoretics, are all necessary under certain conditions. In reference to the diseases of children, Dr. Roberts thinks that bloodletting is admissible in the case of well-fed, full-habited subjects attacked with acute phlegmasiæ, and he combats the views of Dr. J. H. Bennett, of Edinburgh, who, as is well known, is opposed to this measure. Dr. Roberts then passes in review the various other antiphlogistic measures applicable to children's diseases, and he declares his confidence in the powers of mercury to arrest inflammation, if not to absorb its products. Like bloodletting, mercury, he says, may be abused, but it is potent alike for good or for evil.—*Medical Gazette*, New York, April, 1871.

On the Treatment of Syphilis by Hypodermic Injections of Corrosive Sublimate. By R. W. TAYLOR, M.D., of New York.—Dr. Taylor's observations on the treatment of syphilis by injections of corrosive sublimate extend over a period of eighteen months, and were conducted upon about fifty patients, male and female. He adopted this plan of treatment in the various lesions of the whole secondary period of the disease, and he divided the patients into two classes—first, those who had not taken mercury in any form; and second, those who had previously been treated by mercury by the mouth or by inunction. In syphilitic roseola the eruption often disappeared rapidly under the use of the injections, and co-existing symptoms, such as rheumatoid pains and cephalalgia, were relieved at the same time. Dr. Taylor thinks that the exanthem disappears much more rapidly by mercurial injections than when the drug is taken by the mouth, but the rapidity of disappearance is more marked in the recent than in the chronic cases. In the various forms of papular syphilide the eruption disappeared in many instances with astonishing rapidity, the recent cases, however, being far more amenable to treatment than the chronic ones. Both in the papular syphilide and in roseola the eruption disappears most rapidly in the immediate vicinity of the sites of injection. The various nervous symptoms produced by syphilis, such as arthralgia, fugitive rheumatoid pains, and cephalalgia, sometimes yielded very rapidly to the treatment; and Dr. Taylor found that some patients whose sleep had been disturbed for many nights obtained an entire night's rest by one injection of one eighth of a grain of the bichloride at midday, even when they had been previously unable to sleep after a morphia injection. Dr. Taylor admits that mercury taken by the mouth will also allay these pains, but he has never known this

method act so rapidly as the hypodermic injection does. One great advantage of the hypodermic method is the smallness of the dose required, an occasional injection, say once or twice a week, being sometimes all that is necessary in many instances. In cases of mucous patches and condylomata Dr. Taylor does not consider the hypodermic method superior to the ordinary administration of mercury, nor does he think it applicable in brain syphilis or in osseous lesions. The systemic effects of mercury, when injected, are far less unpleasant than when the drug is administered by the mouth, and Dr. Taylor never observed any irritation of the stomach, and only slight stomatitis when the injections were pushed to the extent of two each day. The time required for the disappearance of all the lesions of secondary syphilis by hypodermic injections cannot be fixed with precision, but he thinks that a mean average may be fixed at about six weeks or two months, but if the injections be given daily or twice a day the cure may be accelerated. The quantity of mercury required in the hypodermic treatment may be computed at about four grains of the bichloride, which are equal in efficacy to sixty grains of the protiodide or of blue pill. But while Dr. Taylor claims several advantages for the hypodermic method, he does not assert that it materially lessens the percentage of relapses, which indeed will occur under any treatment and under no treatment at all. In testing the value of the hypodermic injections, Dr. Taylor paid great attention to some minute points which he conceives to be of great importance to the comfort of the patient and to the success of the treatment. Among these is the exact amount of water used in the solution, and he finds the best proportion to be twelve drops of water to one eighth of a grain of the bichloride, a greater quantity of fluid being more irritating. In a very able and impartial summary Dr. Taylor specifies in an aphoristic form the advantages and disadvantages of the hypodermic method of treatment, and he admits that while in some cases it is very useful, yet its application is somewhat limited.—*Medical Gazette*, New York, May, 1871.

On the Influence of Alcoholism on Traumatic Lesions.—In a recent discussion on the influence of alcoholism on traumatic lesions at the Académie de Médecine, M. Béhier stated that this influence, in his opinion, was disastrous, and he adds that the same observation was applicable in the case of internal maladies. The influence in question was not to be regarded as an instance of poisoning, and, independently of the acute symptoms of alcoholism, the different morbid results observed in persons addicted to drink are to be referred to the changes produced by alcohol in the different organs of the body. These changes are of different kinds, according to the period of the disease; and in order to estimate the influence which may be exercised by alcoholism on diseases and on wounds, different phases of alcoholism must be distinguished. At first, alcohol produces transient congestion, and hence it stimulates; at a more advanced stage it induces sclerosis of certain organs, the symptoms varying according to the organ attacked, as in the liver, signs of obstruction of the

vena portæ, in the nervous centres, trembling, dulness of the senses, loss of memory, local paralysis; and at a still more advanced stage it induces fatty alteration of the tissues and steatosis of different organs. Sclerosis and steatosis represent, according to M. Béhier, the important terms of the influence of alcoholism, and these lesions constitute in the economy a condition of degradation which sensibly lessens the force of resistance to depression produced either by diseases or injuries. The alterations caused by prolonged alcoholism are very extensive, steatosis having been demonstrated in the glands of the stomach, in the liver, in the kidney, in the heart and other muscles, and even in the blood. When disease or injury is super-added to these conditions, a series of symptoms is observed which cannot be referred either to ataxia or adynamia, or to putrefaction, although the depression of the system is analogous to the last-named state. The question, however, may be asked, how can it be ascertained that the economy has arrived, under the influence of alcohol in excess, at the period of the organic alterations just mentioned? M. Béhier considers the answer a difficult one, but he points to a coincidence which deserves notice, namely, an excess of fat in the areolar tissue, and he adduces two facts which seem to be proved:—1. An excess of fat in the heart, the mesentery, and some other regions, in persons addicted to alcohol; and 2, the marked *embonpoint* of subjects in whom the serious symptoms in question are manifested when they suffer from disease or injury. Delirium tremens belongs, according to M. Béhier, to a different and less advanced stage of alcoholism. Hence alcoholic preparations which are useful in cases where delirium tremens accompanies acute maladies are not advisable when it is necessary to treat symptoms arising from a state of general steatosis, the last being a true organic lesion in which therapeutic treatment of any kind is of very little avail. M. Verneuil, in the same discussion, differs in opinion from M. Béhier in some respects, and he doubts whether the alcoholic diet is really the cause of the severe cerebral symptoms often observed after wounds, and he thinks that this cause is rather to be sought in the seat, the nature, or the stage of the injury, the cerebral functions being disturbed either directly by the situation of the wound, or by alterations of the blood (as by anæmia or by infection), or through the medium of the nervous system. He thinks that there is no specific remedy for delirium tremens, but that the treatment must be directed according to the conditions presented by the brain and the other organs. The agents which have succeeded best are alcohol and its derivatives, tonics and stimulants, in cases of infectious delirium; and, opium, bromide of potassium, and chloral, in delirium.—*Archives Générales de Médecine*, January, February, and March, 1871.

On the Reduction of Temperature in Fever Patients by the use of Ice-Pillows. By Dr. W. LEUBE, Medical Superintendent of the Fortress of Ulm.—Dr. Leube has lately treated several cases of severe fever occurring among the French prisoners of war in Ulm, and in typhus he employed the cold bath; but this mode of treatment

could be only partially adopted, because the patients strongly resisted it. Dr. Leube therefore determined to supersede the cold baths by another method of refrigeration, which would be no less effectual, but would be less troublesome to the patients. The application of ice-bags might be supposed likely to reduce the temperature, but it was found practically that when even two or three ice-bags were employed there was no remarkable diminution of the animal heat. He therefore endeavoured to improve the apparatus for local refrigeration in two directions, namely, by extending the surface of the cooling medium, and on the other hand by lowering the degree of cold, and these two objects Dr. Leube thinks he has effected by means of his "cold pillows," which he describes. They consist of two large four-cornered pillows, which are filled with a freezing mixture consisting of ice and salt, a combination which he finds much preferable to any other freezing agent. The ice is introduced in a pounded state into the pillows, and common salt is afterwards added, and then the opening by which they were admitted is tightly closed. The pillows are placed lengthwise along the bed, one, which is the larger, for the legs, and the other for the back of the patient, then a caoutchouc covering is laid over the pillows, and afterwards an ordinary sheet. After these preparations the patient is laid on the "cold mattress," and two bands are placed across him, one over his legs and the other over his navel, the objects of this arrangement being to keep the body close to the bed-clothes and to prevent the involuntary movements of the legs which sometimes occur during the treatment. Lastly, the counterpane and the blankets are laid over the body. The patient at first finds the coldness of the bed-clothes very pleasant, and even afterwards the feeling is by no means so disagreeable as that experienced on entering into a cold bath. In four patients in whom Dr. Leube tried the effects of the cold pillows no opposition was manifested by them, although they lay on them from an hour to an hour and a half. In only one of the eight experiments made the patient begged to have the cold mattress removed; and, on the other hand, some patients who had alternately tried cold baths and the ice-pillows, begged to be placed on the latter instead of using the former. After the lapse of an hour or an hour and a half there was slight shivering, which result is to be expected from any effectual refrigerating process, and after this period Dr. Leube usually caused the pillows to be removed, not because they were disagreeable to the patient, or because uncomfortable symptoms presented themselves, but on account of theoretical considerations, which he afterwards specifies. The experiments hitherto made by Dr. Leube gave the uniform result, that, in a bodily temperature of 40° C. and above, the treatment by the ice-pillows reduced the temperature in the rectum 1° to 2° in from one to two hours. Dr. Leube gives the particulars of the cases in which he tried this method of treatment, noting the temperature in each case, and also the pulse and the respiration, and other particulars, and he concludes his paper by observing that the ice-pillows may constitute a very useful apparatus for refrigeration, although he does not conceal his opinion that much improvement

may be necessary in the application of the freezing mixture, the construction of the pillows, &c.—*Deutsches Archiv für Klinische Medizin*, Leipzig, May, 1871.

On the Therapeutical Uses of some of the Plants of Canada. By A. A. HENDERSON, of MacGill University.—Among the plants of Canada which appear to possess medicinal virtues are a kind of *Boletus* or mushroom, growing on the American larch; a kind of *Lycopodium*; and the Canadian mallow. The first, which is a fungus, and is known in Canada by the name of the pine-apple or bitter-apple, grows on the trunk of the larch, and dies when the tree dies, but when obtained from the green tree it shows every sign of life. According to the observations of Mr. Henderson, this fungus acts as a tonic and an emmenagogue, and imparts a bitter taste to the tongue, and increases the flow of saliva. It appears to have been employed with success in Canada by persons who wish to overcome the habitual use of tobacco, for the fungus causes the tobacco to have such a disgusting taste that the habit of tobacco-smoking is thereby discontinued. As a tonic it is employed as a native remedy, steeped in alcohol, and is considered stomachic, and is largely used for this purpose in Upper Ottawa. As a remedy in amenorrhœa it has long been known in Canada, but the fact has been confined to the knowledge of a few persons, chiefly old women, who kept its virtues secret in order to derive profit from its medicinal use. Mr. Henderson, however, gives some cases in which he knows that it has been successfully employed in this complaint. It is given in the form of powder, taken in milk or water. *Lycopodium* has been found efficacious in derangement of the heart's action, produced by weakness or irritability of the nervous system, and its effect is said to be instantaneous, a feeling of quiet and comfort being produced. The Canadian mallow appears to have been usefully employed as a means of reducing abnormal muscular contractility, or, in other words, as a local application in certain forms of rheumatism the result of cold and wet, where the flexor muscles of the limbs become contracted and painful.—*Canada Medical Journal*, March, 1871.

On the Therapeutical Applications of the Hydrate of Chloral. By Drs. LEAVITT and BEAUCHAMP, of the United States.—Dr. Leavitt draws attention to the successful employment of the hydrate of chloral in singultus, and he gives the history of one case and alludes to two others which were cured by this drug. The principal case recorded is that of a gentleman, aged 60, who had long suffered from an obscure disease of the nervous system, and whose sufferings were aggravated by the supervention of an obstinate and distressing hic-cough. Ether, bromide of potassium, musk, camphor, &c., were tried in vain, but the chloral hydrate, used in solution, in five-grain doses, almost immediately arrested the singultus, and never afterwards failed to counteract the spasm in a most satisfactory manner. Dr. Beauchamp records four cases in which the chloral hydrate was successfully employed. The first was one of delirium tremens, occur-

ring in a stout, athletic man, and characterised by very violent symptoms and complete insomnia, and in whom the administration of the hydrate, in half-drachm doses, induced sleep very speedily, and eventually removed all the symptoms. The second case was one of insomnia accompanying epilepsy, for which the hydrate was given, in the dose of a scruple, at bedtime, and by this means the restlessness was overcome. The third case was one of convulsions, occurring after labour, in a young and otherwise healthy lady, and in whom the hydrate was given successfully in half-drachm doses. The fourth case was one of mental excitement, amounting to delirium, occurring in a young man. The chloral hydrate was given in scruple doses at bedtime, and at first it produced nausea and vomiting, but subsequently its use was followed by quiet sleep and a removal of all the symptoms.—*American Journal of the Medical Sciences*, April, 1871.

On the Employment of Hydrate of Chloral in Hydrophobia. By Mr. H. W. ELLIS, of Doncaster.—In the early part of the present year Mr. Ellis was called upon to attend two cases of hydrophobia, occurring in two children bitten by the same dog and at the same time. One case was that of a little boy, in whom the symptoms appeared three weeks after the bite. The other case was that of a little girl, in whom the symptoms appeared almost simultaneously with those of the boy. The features of the two cases were pretty nearly the same, and the results were alike fatal; but Mr. Ellis, with the knowledge that all treatment was inefficacious in obtaining a cure, resolved upon trying the hydrate of chloral, in the hope that it might in a measure control the violence and excitement which usually characterise the advanced periods of the disease. In this expectation he was not disappointed, for the drug mitigated the symptoms very considerably. The remedy was administered in doses of a teaspoonful of the syrup, diluted with a little water, every two hours, and subsequently every hour; and though there was, of course, considerable difficulty in the effort to swallow, it was believed that most of the medicine reached the stomach.—*British Medical Journal*, May 6th, 1871.

On a Case of Lead-poisoning treated with Iodide of Potassium. By Dr. HENRY THOMPSON, of the Middlesex Hospital.—The patient was a painter, aged 26, who was admitted into the Middlesex Hospital, suffering from advanced symptoms of lead-poisoning, namely, frequent attacks of colic, drooping of the hands and fingers, wasting of the muscles of the arm and forearm, blue line along the gums, &c. Iodide of potassium was administered, together with tincture of nuxvomica, and quinine and iron; compound camphor liniment was applied to the hands and arms, and Faradisation was employed on the outer aspects of the arms and forearms. The important point in the progress of the complaint was, that some days after the adoption of the treatment, *iodism* suddenly supervened, characterised by headache, injected conjunctivæ, epiphora, coryza, and great prostration. The use of the iodide was therefore discontinued until the patient's condition im-

proved, and then the salt was administered again, and again aggravated symptoms of iodism presented themselves, and all medicines were suspended except opium. Again, however, the patient improved so much that the iodide was resumed, till another explosive attack of iodism supervened, characterised by severe headache, a pulse of 120, a white, coated and dry tongue, colic, and prostration, but no remarkable coryza. There was now a marked improvement in the movements of the hands and fingers. Dr. Thompson finally discontinued the iodide, and eventually the patient improved so much that he was sent to a convalescent hospital. In his remarks on this case, Dr. Thompson draws attention to the fact that iodide of potassium not only precipitates lead as an iodide, but that it also unites as iodide of potassium with iodide of lead, forming a kind of plumbiodide of potassium. In the case of his patient he thinks he produced, on three occasions, not only iodism, but *iodo-plumbism*, and the last outbreak of iodism was followed by marked amendment; and the case probably affords an illustration of the power of iodide of potassium in dissolving lead and eliminating it from the system in cases where the whole frame is saturated with the poison.—*Ibid.*, April 8th, 1871.

On the Treatment of Hyperpyrexia, as illustrated by Acute Rheumatism, by Cold Applications Externally. By Dr. WILSON FOX.—Dr. Fox relates two cases in which entire recovery ensued under the use of the cold bath, after the temperature had risen in one patient to 110° and in the other to 107.3° . In one case there was deep coma, in the other delirium; but these symptoms disappeared in both after the first reduction of temperature. In the first case there was moderate pericarditis; in the second there was a very large pericardial effusion and also a double pleuro-pneumonia, previous to the rise of temperature and the employment of the cold applications. In both cases quinine had been given in large doses without effect in checking the increase of heat, but the temperature was lowered by the cold bath and other cold applications, such as ice to the spine and packing in wet sheets. Dr. Fox remarks that recovery in acute rheumatism rarely, if ever, takes place after the temperature has risen above 106° , and that in all previously recorded cases death occurred within a few hours after 107° had been reached. No other treatment than the application of cold externally had been shown to be capable of averting the rapid rise of temperature, which took place in different cases after bleeding, and the use of digitalis, veratria, opium, calomel, and large doses of quinine. He therefore considered that the application of cold externally was the only plan that could be pursued with success. Of all the methods employed for the reduction of temperature, total immersion in a bath of from 60° to 80° was the most efficacious, but the proceeding required caution. The wet pack, frequently changed, stood next in order of utility; the ice-bag to the spine had least power, but might be used when the rise of temperature was slow.—*Paper read before the British Medical Association*, August, 1871.

CHRONICLE OF PHYSIOLOGY.

By HENRY POWER, F.R.C.S., M.B. Lond.,

Examiner in Physiology and Comparative Anatomy in the University of London, Senior Ophthalmic Surgeon to St. Bartholomew's Hospital.

BLOOD.—CIRCULATION.

1. W. PREYER. *Neue Blutkrystalle*. ('Centralblatt für die Medicin. Wiss.,' No. 4, 1871.)
2. P. Q. BRONDGEEST. *Over ongekleurde Kristallen in het bloed van bevroren Kikkvorsehen*. On the presence of colourless crystals in the blood of frozen frogs. ('Nederlandsch Archiv voor Genees-en Naturkunde,' 1870, Band v, p. 378, with a plate.)
W. PREYER. *Synthese des rothen Blutfarb-stoffs aus seinen Zersetzungs-producten*. Synthesis of the red-colouring matter of the blood from the products of its disintegration. ('Centralblatt für die Medicin. Wissenschaft.,' No. 10, 1871.)
EXNER. *On the Development of Ammonia in Decomposing Blood*. ('Sitzungsber. d. Kaiserlich. Akad. zu Wien,' Band lxii, Heft. ii, p. 363.)
W. MARCET. *On the Constitution of Blood and Nutrition of Tissue*. ('British Medical Journal,' June 17, 1871.)
3. J. DOGIEL and N. KOWALEWSKY. *Ueber den Blutstrom bei unterbrochener Respiration*. On the blood-current during interruption of the respiration. ('Pflüger's Archiv,' Band iii, p. 489.)
4. R. HEIDENHAIN. *Ueber bisher unbeachtete Einwirkungen des Nervensystems auf die Körpertemperatur und den Kreislauf*. On some hitherto unobserved effects of the nervous system on the temperature and circulation. ('Pflüger's Archiv,' Band iii, p. 504, and 'Centralblatt,' No. 5, 1871.)
5. E. J. M. NOLET. *Vascular Murmurs*. "Zur Lehre der Gefäss geräusche." ('Beiträge aus dem Physiol. Laborat zu Leiden.')

1. M. Preyer remarks that if a watery solution of blood be mingled with its own volume of æthyl æther and a little glacial acetic acid, the superior ethereal layer rapidly assumes a deep brown colour, and exhibits four absorption bands in the spectrum; one between c and d (acetic acid band), and close to c; two between d and e, of which one is near d and faint, and the other near e and stronger; and lastly, a well-marked band between e and f. On slowly evaporating the acid æther, after separating it from the completely colourless hæmoglobin solution, small acicular and frequently bent needles are obtained, either separate or arranged in stellæ. They are doubly refractile. They are of larger size than any other form of blood crystals; they are insoluble in æthylic alcohol and water, but very readily soluble in solution of potash and in dilute acetic acid. They are certainly not hæmin, nor Lech-

mann's hæmatin crystals. He proposes to term them "hæmatoin" crystals.

2. M. Brondgeest found a variable number of colourless crystals in the blood of frogs kept for a variable period at low temperatures. Their form was for the most part prismatic, with very long lateral surfaces and pyramidal extremities, not therefore agreeing with any known form of blood crystals. They are insoluble in water and in ether; soluble in dilute acids and alkalies, and in a 5 per cent. solution of common salt. They are very stable. When dried they may be kept for years unchanged in appearance. They do not form as the blood thaws, but may be seen in the frozen blood. They cannot be obtained from coagulated blood. They are most abundant and best seen in animals that have been frozen into a solid block at a temperature of 4° Cent., and then submitted to a considerably lower temperature. Brondgeest believes them to be derived from albumen.

3. The remarkable increase in pressure in the arterial system occurring with interrupted respiration is generally accompanied by increased vascular contraction. In order to corroborate this, Messrs. Dogiel and Kowalewsky undertook to determine the rapidity of the current of the blood in the larger arterial trunks (for the most part in the crural) with the aid of Ludwig's instrument; and they found that, coincidentally with the augmentation of pressure, there was a considerable diminution of the rapidity of the current, if in curarized dogs artificial breathing was discontinued. In three experiments the rapidity of the current in the crural vein was ascertained, and it was found that the current during dyspnœa underwent alternate increase and diminution of rapidity; but, at the moment of recommencement of the respiration, it constantly underwent an acceleration as compared with the period of dyspnœa.

4. With the object of establishing the fact of the occurrence of certain changes in the temperature of the brain on excitation of sensitive nerves, Heidenhain compared the brain with that of the aortic blood by the thermo-electric method. He convinced himself that the brain constantly possesses a higher temperature than arterial blood, and that their difference increases considerably on excitation of sensitive nerves. Thermometric examination of the blood showed, however, that the temperature of that fluid sank after irritation, in the course of from one to one and a half minutes, as much as one fifth of a degree Centigrade. This observation threw some doubt upon the conclusion that an increase really took place in the temperature of the brain. The unexpected diminution of temperature of the blood was forthwith made the subject of further research. Very fine thermometers were introduced into the aorta, vena cava, hepatic vein, &c., so as to cause little interruption to the blood current. A fall of temperature on excitation of the sensory nerves was demonstrated to take place in all these regions, as well as in the large intestine and the abdominal cavity. This fall of temperature did not take place if the medulla oblongata were separated from the spinal cord. On the other hand, division of the medulla oblongata from the pons was without influence upon it. Direct excitation of the medulla oblongata by electricity, or by suspension of artificial

respiration, lowered the temperature similarly to excitation of the sensory nerves. Hence it would appear that the excitation of sensory nerves acts on the temperature only through the intervention of the medulla oblongata. Coincidentally with the fall of the temperature, the pressure of the blood rises in the arteries. This increase of pressure is usually attributed to a contraction of the small arteries, and consequent impediment to the discharge of blood from the arterial system. Such retardation of the blood current as may be occasioned by excitation of the vagus, evacuation of blood from an artery, or compression of the thoracic aorta, is followed, however, not by a fall, but by a rise of the animal temperature. This is clearly owing to the diminished loss of heat from the surface, which must be the consequence of a retarded blood current. To this also must that post-mortem rise of temperature be referred which is normal in dogs. The loss of heat from the surface occurring from diminished rapidity of circulation is actually less than the diminution in the amount of heat generated. Even when the thoracic aorta is compressed, however, excitation of the medulla oblongata will cause a fall in the temperature of the posterior parts of the body. On the other hand, the diminution in temperature on excitation of the sensory nerves, or of the medulla oblongata, occurs but slightly, if at all, in animals, in a febrile state; whilst the augmentation of the arterial pressure occurs just as in healthy animals. Hence it seems to follow that the fall of the temperature is not the consequence of changes in the circulation. But inasmuch as Heidenhain does not consider that it has been proved that a diminished rapidity in the current of the blood takes place as a result of contraction of the small arteries consequent on irritation of the medulla oblongata, he undertook further researches on this point. He found that, coincidentally with the elevation of the arterial pressure, the pressure in the veins also rose, and the rapidity of the blood current, as measured by Ludwig's instrument (in opposition to the statements of Dogiel and Kowalewsky), also increased even in the larger arterial trunks. Thus it appears that if irritation of the sensory nerves, of the medulla oblongata, and suspension of the respiration, do not retard, but, on the contrary, accelerate the blood current, the depression of temperature produced by these agencies can only be regarded as the result of an augmented loss of heat from the surface. These influences must consequently act the more strongly the colder the surface is. In correspondence with this, Heidenhain finds that in a cold bath of from 57°—61° Fahr., the internal temperature quickly falls, whilst the fall of temperature on excitation of a sensory nerve is remarkably accelerated. In warm baths the internal temperature rises, and this elevation is maintained on irritation; but the influence thus exerted is very slight. If, lastly, the temperature of the bath be higher than that of the animal, an accelerated elevation of the temperature on irritation may be noticed. According to these observations, all the phenomena of altered temperature may be referred to altered loss of heat from the surface, and it is not necessary to admit the view of a direct influence of the nervous system on the production of heat. The experiments on compression of the aorta are not opposed to it, for this does not (on account of the elastic tension of the

arteries) immediately arrest the flow of blood in the parts situated distally to the seat of compression. The small influence exerted by irritation in feverish animals can be sufficiently explained by the higher temperature of the surface of the body in febrile conditions; and that this is correct is demonstrated by the fact that, even in feverish animals, the depression of temperature resulting from irritation of sensory nerves still occurs if the surface of the body is artificially cooled.

4. In his investigations on the production of sounds by the flow of fluids through tubes, M. Nolet used partly caoutchouc, partly metal tubes. The water flowed from a height of about sixteen feet, and the part of the tube ausculted was placed in a groove of a wooden chest. Like Weber and Thamm, Nolet observed that a sound was produced, even when the tube was of equal width, providing the rapidity of the current was sufficiently great. Thus, in a thick-walled tube of caoutchouc, having a diameter of 18.75 millim., a bruit became audible when the rapidity of the current was 1600 to 1700 mm. per second. In general it was necessary, with increased smoothness of the metal wall and diminished diameter of the tube, to increase the velocity of the current in order to obtain a bruit. Greater velocity of the current was required with metal tubes than with caoutchouc, on account of their greater smoothness. The sounds produced appeared to be equally audible in all parts of the tube. A constriction existing at any part of the tube led to the production of a bruit, with a much less velocity of current than in a tube of equal diameter throughout. The sound was produced both before and behind the constriction, but was loudest behind. When the current is passed very rapidly through the constricted portion *frémissement* becomes audible, especially in thin-walled tubes. Dilatations were effected by introducing portions of zinc and caoutchouc tubes into the before-mentioned tubes. The greater the dilatation the swifter the current required to produce a bruit, so that when the dilatation was eighteen times larger than the original tube, the current was obliged to be swifter than when the tube was of uniform size ($\frac{3}{4}$ in. in diameter). It was loudest at the entrance to the dilatation. He attributes the sounds produced in these tubes to eddies. It is thus shown that when aneurisms exceed a certain size, no bruit, or only a slight one, is heard.

DIGESTION—SECRETION.

1. Dr. PASCHUTIN (of St. Petersburg). *Zur Frage über die Wirkung des Speichels auf Amylum.* On the action of saliva on starch. ('Centralblatt für die Med. Wissenschaften,' 1871, p. 372.)
 2. M. EBSTEIN. *Beiträge zur Lehre vom Bau der Magenschleimdrüsen.* On the structure and functions of the so-called mucous glands of the stomach. ('Centralblatt,' 1871, No. 6, and 'Archiv of Mikroskop. Anat.,' Band vi, p. 515.)
 3. G. V. BRAKEL. *Ueber die peristaltische Bewegung insbesondere des Darms.* On peristaltic movements, especially of the intestine. ('Pflüger's Archiv,' Band iv, 1871, p. 33.)
- A. GRIMM. *Experimentelle Untersuchungen über den Brechact.*

Experiments on the act of vomiting. ('Pflüger's Archiv,' vol. iv, p. 205.)

4. VAN LAIR and MASINS. *Ueber einen neuen Abkömmling des Gallenfarbstoffs im Darminhalt*. On a new colouring matter derived from the bile in the intestine. ('Centralblatt für Med. Wiss.,' 1871, p. 369.)
5. Dr. MOSLER (of Greifswald). *Ueber die Function der Milz*. On the function of the spleen. ('Centralblatt für die Med. Wiss.,' 1871, p. 290.)
6. Dr. SIEGMUND ROSENSTEIN. *Ueber die Betheiligung der Nieren an der Harnstoffbildung*. On the participation of the kidneys in the formation of urea. ('Centralblatt für die Med. Wiss.,' 1871, p. 353.)
7. Dr. E. SALKOWSKI (of Königsberg). *Untersuchungen über die Ausscheidung der Alkalisalze*. Researches on the excretion of the alkaline salts. ('Centralblatt für die Med. Wiss.,' No. 19, 1871.)

1. Dr. Paschutin shows that the presence of a large quantity of the products of the metamorphosis of starch does not in any way prevent the action of ptyalin on starch. Thus, for instance, if a small quantity of saliva is added to a mixture containing a large amount of grape sugar and dextrin in solution, energetic conversion of any starch that may be added still continues to take place; and this will even occur if the solution of dextrine and grape sugar have been concentrated by the water bath.

2. Ebstein has investigated, chiefly in dogs, the so-called mucous glands of the stomach. He finds that, whilst the general surface of the gastric mucous membrane and the alveoli are invested by a columnar epithelium, the glands of the pale region near the pylorus, and which were supposed to produce mucus, are lined by an epithelium closely resembling that of the peptic glands of the other parts of the stomach, not only in their microscopic but in their microchemical characters, as well as in the alterations they undergo during digestion. He is therefore of opinion that the glands of the pyloric region produce gastric juice like the true peptic glands, whilst the mucus of the stomach is formed by the cells covering the general surface.

3. M. Englemann, assisted in his experiments by M. G. v. Brakel, contributes a paper to 'Pflüger's Archiv,' on "Peristaltic Movements." Dr. Brinton, it may perhaps be remembered, denied the existence of an antiperistaltic movement, maintaining that when it appeared to exist, as, for example, in the stercoraceous vomiting of hernia, the phenomena observed were due to a powerful peristalsis, which, when the onward movement of the intestinal contents was obstructed, caused a strong axial reversed current, closely simulating an antiperistaltic movement. M. Englemann found, in one case, that when the belly of a cat was opened a few minutes after death the intestines were found at rest; but when a loop was seized and pinched with forceps, a double wave of muscular contraction was excited—one, peristaltic, progressing till it stopped at the ilio-cæcal valve, the other, antiperistaltic, continuing as far as the pylorus, and both travelling at the rate of about an inch and

a half in a second. Two similar waves may be established by the same means in the large intestine, the antiperistaltic one ceasing at the ilio-cæcal valve. Other experiments undertaken upon the ureters and uterus, as well as upon the intestinal canals of many other animals, led to the general conclusion that in all membranes composed of smooth muscular tissue, contractions can be propagated as readily in the antiperistaltic as in the peristaltic.

4. Van Lair and Masius apply the term *sterco-bilin* to the new colouring matter they have obtained from the feculent contents of the intestines. Its presence can be demonstrated by spectrum-analysis. Its absorption band is precisely limited by the lines *b* and *F*, when its solution is sufficiently diluted. If concentrated, however, it extends slightly beyond *F* to the right. Its spectrum relations in regard to acids nearly resemble those of urobiline. Acids do not change the position of the absorption bands, they only strengthen the dark part of the band towards *F*. Alkalies (soda, ammonia, and lime) render the band darker towards the line *b*. *Sterco-bilin* is soluble in water, alcohol, and chloroform; it is quite insoluble in sulphuric æther.

5. Dr. Mosler states that during the past two years he has been engaged with Dr. Schindeler in experimenting on the functions of the spleen, and an account of these observations will appear in a large work they are about to publish, entitled '*Die Pathologie und Therapie der Leukämie.*' In the mean time he communicates the following conclusions deduced from the phenomena observed in about thirty animals from which the spleen had been removed. 1. The spleen is not absolutely necessary to life. 2. After extirpation, or after artificially produced atrophy of the spleen, its functions are performed by the other lymphatic organs. The medulla of the bones appears to discharge an important function in this respect, as remarkable changes, resembling those found in leucæmia, are to be observed in it after extirpation of the spleen. Hyperplasia of the lymphatic glands is not a constant effect. 3. The vicarious activity of these lymphatic organs, which appears to be consequent on many external conditions, is not always complete and perfect in animals deprived of their spleen, since, especially during the first months after its extirpation, or after its artificially induced atrophy, the qualities of the blood are found to have undergone a change. Hence we may conclude that the spleen exerts a direct influence on the formation of the blood, and, as these researches tend to show, on the development both of the white and of the red corpuscles. 4. The spleen exerts no influence on gastric or upon pancreatic digestion. Dr. Silvester, in a pamphlet entitled '*The Discovery of the Nature of the Spleen,*' 1870, has published an ingenious explanation of the apparently anomalous position and relations of this organ.

6. Dr. Rosenstein remarks that the methods hitherto adopted to show whether urea is formed in the kidney itself, or is merely excreted, have been based on the examination of the amount of urea contained by the blood of the afferent and efferent renal vessels, or of the entire blood of the animal previous to and after extirpation of the kidneys, or before and after tying the ureters. These methods, however, are very uncertain, and M. Rosenstein has thought that it would prove a better

plan were the amount of urea contained in the urine to be ascertained before and after the extirpation of *one* kidney. On performing this experiment, the animal being kept on the same diet, he found that, if anything, rather more urea was eliminated after extirpation of one kidney than when both were acting; hence he concludes that urea cannot be formed in the kidney, since it is incredible that hypertrophy of the remaining organ, when one has been removed, should occur in a few hours.

7. Dr. Salkowsky states that observations continued for a year and a half have led him to the following results:—1. In normal conditions the urine is the only secretion by means of which any considerable amount of the alkaline salts is eliminated from the body. 2. Under ordinary circumstances, and in health, the amount of soda eliminated always exceeds that of potash. 3. In febrile conditions the opposite obtains, the quantity of soda eliminated being considerably less than that of the potash, and being even reduced to a minimum. 4. The absolute amount of potash eliminated in fever is three, four, or even seven times greater than in health. 5. There is no reason for believing that the salts of potash are retained in febrile conditions; but in all probability the salts of soda are retained in the system in these states. 6. Under certain circumstances (typhus), notwithstanding free diuresis, a large quantity of the alkaline salts are eliminated by the surface of the intestine. 7. We may admit that it is highly probable that there is an exaltation of the metamorphosis of those tissues in fever, which contain a large excess of potash over soda in their ashes.

NERVES—SPECIAL SENSE—MUSCLE.

1. TH. W. ENGELMANN. *Zur Allgemeinen Muskel und Nerven Physiologie.* Essays on the general physiology of muscle and nerve. ('Pflüger's Archiv,' Band iv, p. 3, 1871.)
2. MICH. LAYDOVSKY. *Ueber die Endigung der Nerven in der Harnblase des Froschs.* On the mode of termination of the nerves in the primary bladder of fishes. ('Centralblatt für die Medicinischen Wissenschaften,' No. 3, 1871.)
3. J. SCHOEEL. *Die Flughaut der Fledermäuse, namentlich die Endigung ihrer Nerven.* The wing of the bat, especially in regard to the termination of the nerves. ('Archiv für Microscop. Anatomie,' Band vii, p. 1.)
4. T. ENGELMANN. *Ueber das Vorkommen und die Innervation von Contractilen Drüsenzellen in der Froschhaut.* On the presence and innervation of contracted gland-cells in the skin of the frog. ('Pflüger's Archiv für Gesammte Physiologie,' Band iv, p. 1.)
5. M. USCHAKOFF. *Ueber die Grösse des Gesichtsfeldes bei Augen mit verschiedener Refraction.* On the extent of the field of vision in eyes of different refractive powers. (Reichert and Dubois-Reymond's 'Archiv,' 1870, p. 454.)
6. IHLDER. *Die Nervenendigung in der Vogelzunge.* The mode of termination of the nerves in the tongue of the bird. (Reichert and

Dubois-Reymond's 'Archiv f. Anatomie und Physiologie,' 1870, p. 238.)

7. E. CYON. *Ueber den Nervus depressor bei der Pferde.* On the depressor nerve in the horse. ('Bulletin de l'Academie des Sciences de Petersbourg,' t. xv, p. 261.)

1. In the paper contained in the first part of 'Pflüger's Archiv' for the present year, Dr. Engelmann discusses the subject of the excitation of muscles and nerves by means of discontinuous electrical currents, and endeavours to establish by fresh experiments the truth of his theory that, after closure of a constant current, only a single wave of excitation proceeds from the negative, and after the opening only a single wave of excitation proceeds from the positive pole into the extra- and intra-polar portions of nerve. He states that recently he has convinced himself that in normal nerves neither the closure nor the opening excitation of a constant current, nor the excitation of a single induction shock, cause the nerve to exhibit any negative variation visible with the galvanometer, providing that the part of the nerve is sufficiently remote from the electrodes of the exciting current to exclude electrotonic effects. On the other hand, all nerves in the abnormal condition, in which they produce closing- and opening-tetanus, exhibit distinct negative variation after closure and after opening of the constant current, but not after excitation with an induction shock. The change in the electro-motor action demonstrates that, like the tetanus of muscle, it is dependent upon the direction and strength of the current in the nerves. He then proceeds to show that when discontinuous currents are applied to nerves or voluntary muscles, in order that a fresh wave of excitation should be propagated, the interruption must be of a certain duration. In the case of the ureter, he had already shown that the interval between the several current-shocks must amount to from one half to a whole second, so slowly are the conditions there re-established, which must be present in order that a new wave of excitation should be generated. Of course this interval would be much less in the voluntary nerves and muscles; but he did not think it could be so immeasurably small as v. Wittich's recent experiments appeared to show. Two years ago Brücke demonstrated that the transversely striated muscles of frogs, under the influence of woorara, are relatively insensible to short interruptions of a continuous current, and he soon after stated that the muscles of animals, to which no poison had been administered, did not respond by any convulsion to an extremely short interruption of a continuous current flowing through its nerves. In the latter case, however, *cæteris paribus*, the duration of the interval must be very much briefer for the muscle to remain at rest. Hence it might be expected that in tetanising the nerves with current-shocks of equal strength, passing in the same direction, and succeeding to one another with extremely short intervals, the muscle would give only a single closure-contraction at the commencement, and an opening-contraction at the termination of the series of excitations. Further, it might be expected that the maximum duration of the interval (t'), in which this still occurred, would be a function of the density of the current. For

Brücke found that with the application of feeble currents contraction failed after longer intervals than when stronger currents were applied. The effect observed must also be dependent on the duration of the individual current-shocks, since this essentially determines the amount of change produced in the nerves. Lastly, different results may be anticipated to occur according to the condition of the nerve and muscle, so that in exhausted, cooled, or dying preparations, the maximal value of t would *cæteris paribus* be augmented. M. Engelmann then describes an apparatus he has constructed, in which a variable number of equally strong current-shocks, passing in the same direction, can be applied in a given time, with intervals that can also be varied at will. The instrument is too complicated to describe without the accompanying plate. His researches are divisible into three series—those on preparations of nerve and muscle; those on muscle poisoned with woorara; and those on nerves. We can only pretend to give his principal conclusions. The first conclusion, in preparations of nerve and muscle, was that the duration of the intervals, through which intermitting excitations acted like continuous currents (the proportion between the duration of the passage of the current and that of the interval remaining the same), was smaller the stronger the current. A second conclusion was that, the longer the duration of the current-shock, the shorter must be the interval between two shocks to make the discontinuous act like the continuous current. As the irritability of the nerve dies out, the duration of the interval may be increased, and the same holds for the state of exhaustion of the muscle and nerve. M. Engelmann coincides with Brücke in considering that when no response is made by the muscle to the interruption of the current the reason is twofold: 1. That the muscle is incapable of reaction to a second shock transmitted through the nerve, if it succeed the first very rapidly; and, 2. That no second wave of excitation or impulse can be transmitted through a nerve if this, in like manner, succeeds the first too quickly. The experiments made with muscles alone under the influence of woorara showed also that if the duration of the interval were sufficiently brief, the excitation was equivalent to a continuous current. The length of interval diminished with increasing strength of the current, and augmented with diminishing excitability of the muscle. From this it is obvious that a certain measurable interval must elapse before a new wave of contraction can be propagated from a directly irritated spot of a muscle, and from a comparison of the recorded times it further appears that the new wave of contraction originates earlier, or with a shorter interval, when the muscle is stimulated through the nerve than when it is directly stimulated by an electrical shock. His final conclusion is that muscle can only be periodically excited through the nerves, and that longer intervals are required than those in which the nerve itself is excitable (by conduction) through the nerves.

2. N. Lavdovsky employs an 0.1 to 0.5 per cent. solution of chloride of gold as a reagent, either alone or with coincident exposure for a few minutes to the action of a very weak solution of acetic acid. He finds pyriform or clavate bodies, which he considers to constitute peculiar apparatus at the ends of the nerves. These bodies are connected at

their base with non-medullated fibres, and have a free rounded peripheric extremity. The non-medullated fibres break up in their interior into a bunch of filaments. They are also surrounded by fine filaments, which form loops around them. They appear consequently to be a modification of the Pacinian corpuscles.

3. J. Schöbl gives a minute account of the termination of the nerves in the wing of the bat. The experiments of Spallanzani and others with blinded bats showed that the membrane of the wing possesses a high degree of sensibility. Schöbl finds the nerves are very numerous, and arranged in successive layers, one of which, containing the large fibres, is central and fundamental. On either side of this, towards the surface, are four layers; the most superficial, which contains the termination of the nerves, being situated in the rete Malpighii. The nerves terminate in two modes—by a delicate plexus of immeasurably fine fibræ, and by terminal tactile corpuscles. These last are situated in the vitreous membrane of the hairs, and coincide with these in number and arrangement. The corpuscles, he thinks, minister to ordinary tactile impressions, whilst the plexus constitutes the recipient surface for impressions of temperature, pain, &c.

4. Dr. T. Engelmann gives a short account of his observations on the innervation of the contractile gland-cells of the skin of the frog. The skin of the frog presents in all parts, he remarks, numerous small glands, the cell lining, which are capable of contraction. The cells consist of a delicate membrane, lined by two scarcely separable layers of cells, of which the external are elongated and flat, and are probably the contractile agents, and the internal more cubical. In the quiescent condition of the gland the cavity is wide, and filled with watery fluid, and the epithelial layers thin. In the contracted, the epithelial layers become greatly thickened, and the contained fluid is expelled. The glands contract under the influence of nervous excitation. Thus momentary electric or mechanical excitation of the peripheric extremity of the divided sciatic nerve, causes a transient contraction of all the glands in the web of the hind feet, preceded by a remarkably short period of latent contraction. The contraction attains its maximum in from half to five seconds, which is immediately followed by relaxation, which is prolonged from ten seconds to several minutes. The contraction can be augmented by applying several electric shocks successively, till the cells may even pass into a state of tetanic contraction, when they become cloudy. Similar effects may be produced by irritating the isolated glands. The glands can be very easily excited reflectorily, slight mechanical, chemical, or electrical stimuli applied to many parts of the body causing a transitory contraction of those of the web of the feet. The reflex irritation is conducted centripetally through the posterior, centrifugally through the anterior roots of the spinal cord. The contractions entirely cease after complete destruction of the brain and spinal cord, and they cannot be reflectorily induced after division of the spinal cord. Spontaneous and sometimes rhythmical movements may be observed. The activity of the motor nerves of the glands is not abolished by woorara.

5. M. Uschakoff finds that the field of vision, in normal or emmetropic eyes, varies in extent in the vertical meridian from 114° — 120° , and in the horizontal from 137° — 142° . In myopic eyes, as a consequence of the concomitant modification of their form, the extent of the field of vision is much less, as determined by an Aubert-Förster's perimeter, amounting only to 92° — 120° in the vertical and from 100° — 140° in the horizontal. There was even greater difference with hypermetropic eyes, the extent in them being 123° — 146° in the vertical and 143° — 174° in the horizontal meridian. The condition of the pupil appeared to exert no influence on the extent of the field of vision.

6. M. Cyon found in the horse nerves closely resembling in their mode of origin the depressor nerve of the rabbit, springing from the vagus and running upwards. When cut, and the central extremity irritated in animals narcotised with hydrate of chloral, a very decided depression of the blood pressure was observed, the pressure falling from 120 mm. of mercury to 20 or 15 mm. After the fall of pressure consequent on irritation the pressure again rose.

7. M. Ihlder, whose observations were conducted under W. Krause's supervision, states that the nerves of the tongue in birds do not end in naked axis cylinders; but that all the constituents of each nerve fibre become greatly attenuated, and that it terminates in a ganglion cell. He also describes certain tactile bulbs the structure of which is intermediate to the terminal bulbs of mammals and the ordinary tactile corpuscles. They differ from the corpuscles of Herbst in the absence of an external sheath; from the cylindrical terminal bulbs of mammals by their transverse striation, which is dependent on the presence of a large number of transversely arranged nuclei; and from the ordinary tactile corpuscles, which they resemble in form and size, and transverse striation in the absence of numerous transversely running nerve-fibres. The tactile bulbs possess only a single axially placed terminal fibre, which ends in a large ganglion-cell at the peripheric pole of the bulb.

M. Nigetiet remarks that Heidenhain long ago showed that the production of acid in active muscles augments with the tension of the muscles during their contraction. In order to ascertain whether the same obtains for other results of the metamorphosis of muscular tissue, MM. Nigetiet and Hepner examined the alcoholic and watery extracts; the former, according to Helmholtz and Ranke, increasing, the latter decreasing, during the period of activity. In correspondence with earlier researches, they found that, on comparing muscles under otherwise precisely similar conditions, but of which one was weighted whilst the other was not, the former yielded more alcoholic and less watery extractive matter than the latter.

MUSCLE.

1. W. OGLE. *On Dextral Pre-eminence*. ('Proceedings of the Royal Medical and Chirurgical Society,' vol. vi, No. 8.)
2. Rev. S. HAUGHTON, M.D. *Three Lectures on the Principle of*

Least Action in Nature, illustrated by Animal Mechanics.
(‘British Medical Journal,’ Nos. 543—547, May and June, 1871.)

HUGO KRONECKER. *On the Laws of Muscular Exhaustion.*
(‘Monatsberichte d. k. p. Akad. d. Wissenschaften zu Berlin,’ 1870, August.)

N. NIGETIET and HEPNER. *Versuche über die Abhängigkeit des Stoffumsatzes in den thätigen Muskeln von ihren Spannung.* Researches upon the dependency of metamorphosis of tissue in active muscle upon their tension. (‘Pflüger’s Archiv,’ Band iii, p. 574.)

1. Dr. Ogle, after showing that the right leg as well as the right arm is used preferentially by many animals as well as by man, and in the latter before any education is begun, endeavours to prove that there must be some structural foundation for right-handedness; and from observations made on various right and left-handed individuals, he has arrived at the conclusion that, like the faculties concerned in speech, those associated with the predominance of the right hand and leg are located in the left hemisphere. The greater development of the left hemisphere he attributes to its receiving a freer supply of blood than the right.

2. Dr. Haughton’s lectures, published in the ‘British Medical Journal,’ contain many points of interest in animal mechanics, expressed in a very lively and attractive manner. His experiments show that 94·7 lbs. per square inch of sectional area is the weight that the arms of a young man accustomed to athletic exercise are capable of lifting. 110·4 lbs. is the corresponding coefficient for the muscles of the legs of a similar class, and 107 lbs. for the muscles of the abdomen. He takes 104 as the mean result. He describes the different forms of muscles, dividing them into the prismatic, the penniform, the triangular, and the quadrilateral, and gives estimates of the amount of friction experienced by different tendons of various animals at several joints. The last lecture is occupied with the consideration of the heart. He finds that the contracting muscle, a single ounce in weight, of the human heart, will lift 20·576 lbs. through the height of one foot in a minute. He finds the coefficient of capillary resistance varies in different animals, being in the sheep $\frac{1}{18}\cdot6$; the dog $\frac{1}{19}\cdot6$; the horse $\frac{1}{39}\cdot3$; the ox $\frac{1}{39}\cdot8$. The lectures will well repay perusal.

REPORT ON PATHOLOGY AND PRINCIPLES AND PRACTICE OF MEDICINE.

BY FRANCIS C. WEBB, M.D., F.L.S.,

Member of the Royal College of Physicians, Physician to the Great Northern Hospital.

Acute Dropsy, Scarlatinal and Idiopathic.—Dr. H. C. Wood, of Philadelphia, writes to combat the theorem of Prof. Hughes Bennett, viz. that “serous effusion or dropsy is always indicative of mechanical obstruction to the return of the blood from the capillaries through the veins.” Wood argues that this theory is not proved, and even not probable; that the evidence, as far as it can at present be made out, shows that a large proportion, if, indeed, not all acute dropsies are due, not to a mechanical impediment to the circulation, but to a peculiar condition of the cellular tissue, whereby its natural secretion or exhalation is enhanced, so that the water may be said to be actively thrown, or drawn out from the vessels. To establish this he endeavours to prove the following propositions:—(1) “There are œdemas local in character”—*e. g.* the œdema of erysipelas—“which cannot be caused by any mechanical interference with the circulation, but which are evidently connected with a state of irritation of the cellular tissue of the part affected, which irritation is sometimes absolutely local in character, sometimes evidently dependent upon a constitutional blood affection.” (2) “That the dropsy of scarlet fever is mostly, if not always, independent of, *i. e.* not caused by, the disease of the kidney.” In proof of this the author quotes a number of authors who describe cases and forms of scarlatinal dropsy without albuminuria. (3) “There are cases of idiopathic general anasarca absolutely resembling those associated with acute desquamative nephritis, and produced by the common cause of the latter, in which, however, there is no disturbance of the kidneys, nor of the heart, lungs, or liver.” This proposition he supports by cases observed by various authors and by himself. (4) “By the use of a certain drug or drugs we can produce general anasarca without albuminuria, to be followed by the presence of both albumen and tube-casts in the urine, if the use of the poison be persisted in, showing that the anasarca and kidney trouble are the results of a common irritant cause, unless it be asserted that the anasarca produces the albuminuria.” The action of arsenic in the production of œdema is here referred to, and observations in support are quoted from Stillé and Weir Mitchell. (5) “The non-albuminous dropsy of scarlet fever is not anæmic in its origin, but dependent upon irritation of the cellular tissue.” The inconstancy of dropsical effusion in anæmia, and the fact that non-albuminous scarlatinal dropsy may attack parts, *e. g.*, the face first, independent of gravity, that it is sudden in its onset and profuse, and that it is cured by remedies which would produce anæmia, are arguments adduced in support of this proposition. The following are the

author's conclusions:—(1) That in acute Bright's disease, whether originating from scarlet fever, arsenical poisoning, or cold, the dropsy is not the result of the kidney disease, but, with the latter, is dependent upon a common cause. (2) That an irritant poison, organic or otherwise, may give rise to dropsy without other appreciable organic disease. (3) That exposure to wet and cold may produce dropsy, without other disease, and that there is, therefore, such an affection as acute idiopathic dropsy. (4) That acute dropsy is mostly, if not always, the result of irritation of the cellular tissue.—*American Journal of the Medical Sciences*, July, 1871.

Leucocythemia.—Dr. Henry Eames reviews the opinions put forward by Virchow and Dr. Hughes Bennett as to the nature and mode of production of leucocythemia. He regards leucocythemia as an idiopathic affection, consisting not merely in an increase of the number of the white cells, but also in a proportionate decrease in the number of the red discs. He subjoins the history of a well-marked case.

R. W—, æt. 35, admitted into Mercer's Hospital, April 4th, 1871. Five years ago was taken suddenly ill in the night with burning, continuous pain in the left hypochondriac and lumbar regions. No rigors, thirst, or heat of surface. He was supposed to be passing a renal calculus, and was treated with opium and a warm bath, which gave temporary relief. But the pain returned, and continued twenty-four hours without remission, and then gradually disappeared. After a time he got better, and was able to resume his work, but was in the habit of vomiting his food. In January, 1870, he caught cold. He had cough, rusty-coloured sputa, and oppressed breathing. The cough left him in three weeks; but he continued to lose strength and weight, and became pallid. After drinking ferruginous waters at Trefriw, in Wales, he was attacked with diarrhœa, and afterwards suffered severely from pain in the abdomen, which became swollen. A splenic tumour was diagnosed, and his blood under the microscope was found to be leucocythemic. His condition on admission was as follows:—Skin yellowish, conjunctivæ clear, lungs and heart healthy; no murmur in veins or arteries; abdomen distended; girth round umbilicus 36 inches; midway between umbilicus and ensiform cartilage 58 inches. A tumour, evidently splenic, extends two inches to the right of the umbilicus, and touches the symphysis pubis. It can be plainly felt behind at the left side of the lumbar spine. The tumour is extremely hard. No splenic murmur; no glandular enlargement. Percussion dulness of liver is not increased. No ascites. Slight anasarca of feet and legs. Suffers from diarrhœa. Urine, 35 oz. in 24 hours; sp. gr. 1015·7; slightly acid. 1000 parts contain only 10·6 parts of urea, the lowest healthy standard being about 15 parts in 1000. The daily amount of urea excreted is 153·125 grs. The average amount for a healthy man of the same size on full diet is about 4000 grs. in 24 hours. The amount of urea, therefore, is much diminished. Blood drawn showed a large excess of white corpuscles. The relative proportion between the red and the white cor-

puseles was estimated at three of the former to one of the latter. "The white corpuscles are large, and nearly all of them multinuclear, thus bearing out Virchow's observations on splenic leucocythemia." Temperature normal. Mental powers uninjured. Suffers from weakness.

Ophthalmoscopic examination by Dr. Fitzgerald:—The fundus oculi presents an orange tint; the margin of the right optic disc is ill defined and striated; the retinal vessels were rather fainter than normal; no white spots or stripes along the sides of the vessels, which have been supposed to depend on the agglomeration of escaped white blood cells; no hæmorrhagic effusion; the left optic disc well defined.—*Dublin Quarterly Journal*, May, 1871.

Diabetes.—In a paper on the pathology and causes of diabetes, after giving a resumé of the views of Bernard, Pavy, and Flint, Dr. G. M. Smith draws attention to the probability that there may be a possible increase of hepatic glycogenesis under certain normal conditions. One of these is utero-gestation and lactation. "It seems to me that the peculiar condition of the breasts towards the close of utero-gestation, and especially during lactation, can excite the liver to increased glycogenesis by a reflex nervous influence, and the animal sugar thus naturally thrown into the circulation, and which under morbid circumstances would be mainly eliminated by the kidneys, as in diabetes, is in this instance chiefly appropriated by the mammæ, and made to subserve the important work of aiding in the nutrition of the infant. The blood of a healthy nursing woman analysed for Dr. Smith yielded sugar; there was no saccharine matter in the urine. Dr. Smith has observed that uterine irritation during gestation may excite augmented hepatic sugar generation by a reflex influence, and he quotes two cases of diabetes accompanying pregnancy. A temporary glycosuria during utero-gestation and lactation has been remarked by M. Blot. After noticing the connection between injuries and disease of the brain and diabetes, Dr. Smith continues:—"As we do not recognise any constant hepatic lesion in melituria, as we do recognise cerebral alterations in some cases, and are aware of instances of sugar formation by a reflex influence, the following definition and classification of diabetes is suggested: *Diabetes mellitus*, a disease of the nervous system, depending either upon *centric* or upon *eccentric* disturbance; by *centric*, implying cerebral lesion; by *eccentric*, referring to peripheral irritation transmitted to the brain, and reflected either to the liver or other parts, inducing the formation of sugar, and likewise generally reflected to the kidneys, exciting excessive diuresis. *Diabetes insipidus*, a disease of the nervous system, depending either upon *centric* or upon *eccentric* disturbance; in this malady the morbid influence, whether reflected or otherwise, being chiefly directed to the kidneys. Dr. Smith notices that out of 80,016 deaths which occurred in New York, only 56 are recorded from diabetes, either as a proximate or a remote cause, or but one death in every 1379. Dr. Smith asks whether the climate of New York, which seems to

favour Bright's disease, is inimical to melituria. One object of his paper is to advocate the restoration of diabetes to the class of nervous affections in which it was placed by Cullen, on the ground that a large proportion of confirmed cases of melituria appear traceable to neurotic causes.—*New York Medical Journal*, May, 1871.

Spontaneous Rupture of the Heart.—In an elaborate memoir Dr. BARTH examines twenty-four cases of rupture of the heart recorded in the 'Bulletin de la Société Anatomique.' Of the 24 cases 7 were those of males and 17 of females. This unusual predominance of the female sex, however, is shown to have depended partly on an accidental circumstance. The age of only two females was less than sixty, one of the exceptions was fifty-eight, the other fifty-two. Of the remaining 22, 17 (6 men, 11 women) were between seventy and seventy-nine years, one man was eighty-one, one woman sixty, another sixty-seven, and there were two aged females from the Salpêtrière whose exact age was unknown. Seven of the subjects were stout, and only two were noted as thin. In two cases habits of drinking were recorded. In twelve subjects the state of health previous to the rupture was recorded. Three were in good health, the others suffered from oppression, palpitations, vertigo, occasional loss of consciousness, &c. Amongst the exciting causes were, mental anxiety in one case; a large meal (2 cases); coffee taken with brandy (1); the effort to rise in bed (2); several of the patients suffered from constipation, and in five death occurred during efforts at defæcation. In the great majority of cases death was sudden; some of the patients fell as if struck by lightning; more rarely life was prolonged for some minutes; in two cases only did life continue for half an hour—in one of these the patient lived an hour and a half after consciousness was lost. The suddenness of death favours the supposition that the rupture takes place *d'un seul coup*, but on examination of the cases it appears that in several of them at least the rent was made at different times, that is to say, by successive division of the muscular layers forming the wall of the ventricle, and death took place on the rupture becoming complete. In all the cases the pericardium contained blood, which, in fifteen cases varied in amount from 200 to about 500 grammes. In one case the pericardium contained more than two litres of black semicoagulated blood. The blood was more or less coagulated. In twenty cases the rent was single, both as to the exterior and interior of the heart. In one case there were two external linear rents situated the one in front of, the other behind the left border, and but one opening in the cavity of the left ventricle. In another there were three external rents, of which two communicated before penetrating the cardiac cavity, the third extended into the cavity alone. In all the cases the rent was in the left ventricle. In five cases the rent was on the anterior surface of the heart, near to the right ventricle; in five it was on the anterior surface towards the border of the left ventricle; in three on the border itself; in eight partly on the posterior surface, and partly on the border; in one on the middle of the posterior

surface; in one at the apex of the left heart, in one at the apex of the heart, the situation not precisely noted. In half the cases the rent was in the middle of the heart (longitudinally); in four cases it was near the base; in one third it had taken place near the apex. In eighteen of the twenty-four cases, the heart was loaded with an abnormal quantity of fat, forming a thick layer on the surface or infiltrating the tissue, and under the microscope the cardiac tissue presented yellow granulo-fatty degeneration, with more or less marked effacement of the striation of the muscular fibres. In several of the cases the cardiac arteries were found ossified, their walls encrusted with calcareous matter, and their calibre diminished. Of the twenty-four cases there was not one in which there was an absence of all anatomical alteration. In one case only it is noted that the tissue of the heart was healthy, but in this the coronary arteries were ossified and the arch of the aorta was considerably narrowed. The principal anatomical alterations were interstitial hæmorrhages in the wall of the heart and fatty infiltration of the muscular tissue. Other anatomico-pathological alterations observed in certain of the cases are described as alterations of colour of the tissue—yellowish or grayish—partial thinning and alterations of consistence of the walls of the heart, partial friability and ramollissement. In one instance M. Durand Fardel attributed the rupture of the heart to inflammatory softening; and some of the cases show that rupture of the heart may be favoured by the effects of antecedent inflammation, perforations, and erosions of the pericardium, opacities, thickening and erosions of the endocardium, and by ulceration. Of this last cause a remarkable instance is given, in which the ulceration had destroyed the columnæ carneæ, and had extended as far as the external surface.—*Archives Générales de Médecine*, Janv., Fev., Mars, 1871.

Varicose Aneurism.—Dr. STOKES, at a recent meeting of the Dublin Pathological Society exhibited a remarkable specimen of this disease, in which an aneurism of the aorta, originating in one of the sinuses of Valvula opened into the right ventricle, immediately below the valves of the pulmonary artery. The patient was a man, æt. 31, of intemperate habits. He suffered from anasarca and severe dyspnœa, and oppression in the region of the heart. The liver extended below the ribs nearly down to the crest of the ilium. The area of cardiac dulness was much increased, and the impulse correspondingly diffused. There were three distinct murmurs—two at the base, a loud harsh murmur of exodus, and a faint badly-marked regurgitant murmur. Tracing down towards the apex, this murmur became more indistinct, until the apex was reached, where it was very distinct, of a blowing character, and systolic in time. No visible pulsation of the arteries; collapsing pulse not well marked. Fremissement could be distinctly felt over the base of the heart, following the cardiac impulse. At the seat of this fremissement the patient was sensible of a peculiar sound, which he compared to that of “a woman churning.” At the post-mortem the heart was found

enlarged, right cavities distended with blood; left ventricle much hypertrophied. Mitral orifice and the two curtains of the mitral valve were normal. "This is a remarkable fact, and would serve to indicate that where a well-marked basic murmur exists along with a murmur at the apex, the existence of the latter does not necessarily establish the fact that the mitral valve is diseased. It is not by any means improbable that, in many of these cases, the murmur heard at the apex of the heart is merely the basic murmur conducted by the body of the ventricle itself." The aorta was greatly dilated, and its coats diseased. From its anterior wall, close to the semilunar valves, sprang an aneurism nearly the size of a tennis ball; it may be said to have arisen from one of the sinuses of Valvula. In its posterior wall was a rounded opening, through which a goose quill could be passed. This aperture established a communication between the aneurismal sac and the right side of the heart, for the opening conducted into the conus arteriosus of the right ventricle, immediately below one of the semilunar valves of the pulmonary artery. The sac which did not contain coagula, lay within the pericardium. A short time before death there was extreme lividity of countenance. Dr. Stokes observes that the direction taken by the aneurismal tumour downwards towards the heart confirms an observation made in 1836, by Prof. R. W. Smith. He observed that in cases of aneurism, arising from the aorta within the pericardium, or close to the sigmoid valves, the tumour has a tendency to pass downwards, and encroach upon the cavities of the heart. He assigns as a reason, that the opening of the artery into the sac is placed within the influence of the retrograde flow of blood, the weight and impulse of which must tend to direct the tumour downwards. This is the second case observed by the author in which an aneurism, springing from the aorta, close to its origin, has simulated permanent patency of the aortic valves.—*Dublin Quarterly Journal of Medical Science*, August, 1871.

Diaphragmatic Pleuritis.—Dr. Thomas Hayden has reported and collected several cases of this comparatively rare affection. From the cases he has witnessed he draws the following conclusions:—Diaphragmatic pleuritis is characterised by, 1. Sudden and severe pain in either hypochondrium, extending in the line of the costal cartilages, generally likewise down the corresponding side of the abdomen, occasionally upwards to the tip of the shoulder, and aggravated by movement of body, full breathing, cough, and vomiting. 2. Shallow and thoracic breathing, dorsal decubitus, and comparative freedom from pain in the state of rest. 3. Absence of febrile action in mild cases uncomplicated by general pleuritis, pneumonia, peri-hepatitis, or other form of acute inflammation; and in severe cases, whether complicated or not, symptoms of collapse. 4. Partial or complete suspension of respiratory sound in the base of the lung on the affected side, and faint frottement, or rustling sound, audible with respiration. 5. Inability to swallow; nausea and vomiting, of occasional but rare occurrence, and confined to the most aggra-

vated cases. 6. Displacement of the diaphragm upwards in recent cases unattended with suppurative inflammation, but downwards where the antecedent inflammation has proceeded to the formation of pus. 7. Resistance of the symptoms to active treatment, and yielding to dry cupping, the local use of belladonna, and opium internally. Where *upward* displacement of the diaphragm exists, it may be concluded that suppuration has not taken place, that the muscular structure of the diaphragm has not been implicated in the inflammation, or paralysed; and that *pro tanto* the case is one of favorable augury. "I have not witnessed examples of inability to rest save in the sitting and stooping posture, risus sardonius, or delirium; and am disposed to regard these alleged symptoms of phrenic pleurisy as of equivocal value, and strictly accidental."—*Dublin Quarterly Journal*, August, 1871.

Muscular and Cutaneous Anæsthesia, the result of Cold.—Dr. A. Corlieu records the case of a soldier, æt. 23, who was brought to the ambulance "du presbytère Saint-Roch," on Jan. 27th, 1871. When received he was unable to speak, and had lost power over the whole of the left side. Seven days previously, when lying in his barrack, his left side had been exposed to cold. This was followed by continual formication in the left leg, then in the side and left arm. His voice became weak, and afterwards there was complete aphonia. On examination he was found not to be paralysed; but there was complete anæsthesia of the whole of the left side. He was insensible to the touch (anæsthesia), to pain (analgesia), and to heat or cold (thermo-anæsthesia). The cutaneous sensibility was abolished, but the motility was perverted. In whatever position the arm or leg of the left side was put it remained, despite efforts made by the patient to change it. He used his right hand to put the left limbs in a position of rest. The case was not therefore a case of catalepsy, but of muscular anæsthesia, characterised by want of voluntary power over the muscular contractions. The eyelids were shut on both sides, and the patient could not open them; when opened he made a sign that he could not see. Phonation was abolished, but the movements of the tongue were free. Respiration and circulation were not affected. The patient was hungry; there was no constipation, no albumen in the urine. Under treatment, by warm stimulating drinks and applications, nourishment, acetate of ammonia, the patient first recovered sight on the right side, then on both sides, and then began to recover sensibility and motility in the left leg; but the left arm continued in the same state, and there was loss of voice. He ultimately, however, got well under faradisation. The author proposes for this form of muscular and cutaneous anæsthesia the name of *Néurose Cataleptiforme Hémiplegique*.—*L'Union Médicale*, Sept. 12th, 1871.

Tumour of the Pineal Gland.—Dr. P. Blanquique records the case of a man, æt. 39, who was admitted into La Charité on September 7th, 1869. His illness dated from September, 1868, when he began to suffer from continual and increasing pain in the head. In

February, 1869, he found that the acuteness and extent of his vision was becoming diminished. He then went into the Beaujon hospital, where he had several epileptiform attacks, and his headache resisted all treatment. In La Charité he was treated with iodide of potassium, but the epileptiform attacks occurred every two or three days. During the attacks he entirely lost consciousness; his face was congested, his muscles stiffened; otherwise he had a good appetite, his digestive functions were perfect, and his cerebral faculties intact, except his memory, which was defective. In January, 1870, he was obliged to keep his bed, his limbs would not carry him but he was not paralysed; he was quite blind, the pupils largely dilated. The fundus oculi, examined with the ophthalmoscope, showed no alteration of the circulation, but there was atrophy of the papilla; the eyes were drawn downwards and to the right, but there was no paralysis of the motor nerves of the eye. Violent pain in the back of the head and around the orbits. Bromide of potassium seemed to relieve pain and produce sleep at night; but the attacks became quotidian, feebleness increased, the intelligence became obscured. He died convulsed, on February 8th. Autopsy, thirty-six hours after death.—Thoracic and abdominal organs healthy. On the upper surface of the brain were about a score of small reddish vegetations, situated on two symmetrical lines two centimetres from the falx cerebri. The dura mater thickened, pearly, adherent to the brain in the situation of the vegetations. Careful examination showed that there were protrusions of the cerebral substance through the dura mater in the situation of the Pacchionian corpuscles. The convolutions were flattened, the sulci filled. At the base there existed a large serous effusion which, by compressing the brain, had flattened the convolutions, and had overcome the resistance of the dura mater at the weakest part, that is to say, in the situation of the Pacchionian corpuscles. The central and lower parts of the brain were softened by imbibition. The lateral ventricles and the fifth ventricle were much dilated and filled with a clear yellowish fluid; a finger could be introduced into the foramen of Monro. The choroid was very vascular; the veins of Galen were gorged with blood, especially the right vein; the pineal gland was enlarged into a tumour of the size of a pigeon's egg. On cutting it the tumour presented some calcareous granulations and hæmorrhagic patches. This appearance is that presented by tumours of the choroid described by Virchow under the name of *psammomes*. The tumour was not exactly in the middle line, it extended more to the right than to the left; it compressed the veins of Galen and the quadrigeminal bodies, especially the posterior. Examined microscopically by M. Graucher, after hardening in chromic acid.—A section showed various colours, reddish-brown, deep yellow, gray. A large quantity of small cells of all forms, round, elongated, brilliant, and refracting (nerve cells or degenerated epithelium?) are aggregated round processes of connective tissue and form the fundamental elements of the tumour. The accessory elements are large vessels gorged with blood, hæmorrhagic islets sown with crystals of hæmatoidin, and amy-

loid concretions forming small globules visible to the naked eye. The tumour seemed to be an hypertrophy of the pineal gland, presenting the normal elements and general character, with the addition of more numerous concretions and disseminated hæmorrhages. The microscope showed that the small tumours on the convexity of the brain consisted of cerebral substance. The author concludes that a tumour of the pineal gland does not give rise to any symptom proper to itself; the symptoms were those which are common to cerebral tumours.—*Gaz. Hebdomad. de Méd. et de Chir.*, Sept. 15th, 1871.

On the Growth of the Nails as a Prognostic Indication in Cerebral Paralysis.—Dr. S. Weir Mitchell observed in a lady, æt. 56, the subject of an attack of left hemiplegia, from which she recovered to a great extent, that during her illness the finger nails on the left side, which were previously healthy and rather large, became marked with deep serrations, crossing from side to side, and about one line apart. The peculiarity remained whilst she was under Dr. Mitchell's care, and the growth of these nails was much slower than that of the nails of the healthy arm. In consequence of this observation, Dr. Mitchell determined to study the nail growth in similar cases. The first case he relates was that of a gentleman, æt. 47, who had a severe attack of apoplexy. "The morning after his attack I stained four of the nails of the palsied right hand down to the lower edge with nitric acid, hoping thus to learn whether they would grow as fast as those of the other side. To my surprise, while the left healthy nails grew as usual, the right nails did not grow at all during three weeks. Then, and while the arm was throughout still motionless, the nails began to grow, as was shown by a line of white below the tinted portions. Within a week after this the fingers became controllable by the will, and gradually the whole hand, and then the arm was restored, so as to perform any except the most delicate tasks." The third case related by Dr. Mitchell was that of a gentleman who, after great mental excitement, became paralysed as to motion only in the right arm and leg on October 16th. On the fourth day after the attack Dr. Mitchell stained the nails, the hand being "moveless." Not the slightest growth took place on the palsied side until November 2nd, when, seeing a line of white above the quick, Dr. Mitchell predicted that within a week he would begin to move the limb. On the fourth day after the thumb recovered some slight power, and the rest of the limb rapidly followed it, so that every muscle was under control on November 9th, although for some time the extensors of the fingers moved with difficulty, because of the continued, but lessening, spasm of the flexors. The author remarks that in old cerebral palsies the nails are often deformed, and even the muscles may undergo changes which are possibly due to the neural sclerotic alterations which sometimes come on after the part has been long disused. These changes are the direct result of isolation from special trophic influence. In recent cerebral palsies it is more remarkable that the nails should suffer in their nutrition, as there is no muscular atrophy. In parts whose nerves are severed the

nails grow as usual. In partial nerve wounds we meet with clubbing and serration. Dr. Mitchell thinks that the injury to the brain exerts an inhibitory influence, and that the fact is in favour of the existence of nutritive nerves. He does not admit that the checked growth is due to contraction of the vessels from a vaso motor nerve impression, because no conceivable amount of such spasm could last long enough without altering the tint of the nail. To test this view, however, he faradized two of the nails daily, causing great pain, and intensely flushing them, but no more growth took place in those nails than in the others. In one case a lower temperature of the affected hand was noticed during the attack, but in hands cut off from all nerve connection the temperature is still lower, and yet the nails grow.—*Trans. Col. Phys., Philadelphia, American Journ. Med. Science*, April, 1871.

Case of Sclerosis of the Feet and Legs, with complete Anæsthesia, but not attended by any Locomotor Ataxia.—Dr. Wm. Pepper reports the case of a man, æt. 45, who seven or eight years ago was seized with an attack of inflammation of the feet, involving the skin of the feet and of the legs up to about a hand's breadth below the knees. It was attended with redness, swelling, some heat, and tingling pain. The heat subsided after a time, but the swelling persisted, and he noticed soon afterwards that the sensation of his feet was becoming impaired. From about three inches below the tubercle of each tibia throughout the leg and over the entire surface of the feet the skin became blue; its consistency was greatly increased, its temperature reduced, and the whole member had a hidebound indurated character. The nails became brittle, broken, and discoloured, and fell off. There was a tendency to ulceration about the joints of the toes, and cutaneous sensibility was absolutely destroyed. To so great an extent was this the case that the man took pleasure in lighting large pieces of paper, and then trampling them out with the soles of his bare feet, and in doing this he said that he was not able to feel the slightest sensation of warmth. A galvanic current, derived from thirty cells, was passed through the skin of both legs and feet, the positive pole being maintained in contact until the entire derm was destroyed by the caustic action of the current without arousing the slightest sensation. He might be struck a forcible blow with a cane over the exposed surface of the tibia without his being conscious, except by the shock to his body, of being touched. The toes might be violently trodden on, and he would not be aware of it. The anæsthesia of the soles of the feet was even more extreme than that of the leg. Sclerosis of the legs going on to entire extinction of sensibility is very rare. Despite, however, this complete anæsthesia, the man had not in any degree lost the power of co-ordinating the muscular movements. He could walk perfectly with his eyes closed or on the darkest night, although he was unconscious of touching the ground, and felt as though walking through mid-air. The case contrasted with cases of progressive locomotor ataxia, due to sclerosis of the posterior columns of the cord, in the fact that the

power of co-ordinating the muscular movements was perfectly retained, although the transmission of sensory impressions from the skin was interrupted. The case therefore seems to have an important bearing on the question of the existence of a true muscular sense, independent of the information derived from the tactile sensations of the cutaneous surface. In some cases of progressive locomotor ataxia, even though the anæsthesia of the soles of the feet is not complete, the patient is not able to walk or even stand when his eyes are closed. The case, therefore, affords the strongest pathological argument for the existence of a muscular sense. The man ultimately recovered sensibility rapidly, apparently in consequence of the repeated application of blisters to the diseased surfaces, which probably had the effect of stimulating the derm and the subcutaneous tissue, and of inducing the absorption of the partially organised lymph with which the tissues were infiltrated. The author regards the case as one of sclerosis of the skin in which the proliferation of lymphoid cells had actively advanced, and had induced such pressure on the cutaneous nerves as to cause entire anæsthesia of the affected surfaces.—*American Journal of the Medical Sciences*, July, 1871.

Plica Polonica.—M. Urbanowicz, a Polish practitioner, has communicated a thesis on *Plica Polonica* to the faculty of Strasbourg. He examines the claims of *plica polonica* to be a special malady transmitted by contagion, or to be a diathesis which may be hereditary. He finds that the plica never attacks persons with short hair; that it is a simple matting (*intrication*) of the hair produced by negligence, and at times also by the employment of agglutinative substances. It presents no characteristic group of symptoms, and runs no characteristic course. The formation of plica does not produce any amelioration of other diseases which accompany it; it is not a crisis, and, contrary to prevalent prejudice, the section of the plica is not injurious, but, on the contrary, is the first step in its rational treatment. Guensberg, in 1843, believed that he had discovered a peculiar mycoderm, the trichoma phyton, in plica; it has been described, and has been used to give plica a scientific existence, but the trichoma has not been again found, or it has been proved identical with that of herpes tonsurans. The author considers it proved that in the *intrication pliqueuse* there is no change in the bulbs, roots, or shafts of the hair, and that any cryptogams and parasites that are produced are developed secondarily.—*Archives Générales de Médecine*, Janv., Fev., Mars, 1871.

Rhinoscleroma.—Hebra reports a case of this affection. The disease commenced with an induration of the skin of the under part of the nose and the upper part of the upper lip. The affected part was hard to the touch, and the integument of a brownish-red colour. The patient felt as if the opening of the nares was diminished. There was no pain. The progress of the disease was slow, and the character of the induration remained unchanged. There was no formation of pus or ichorous matter. The different portions of the

tegumentary indurations grew in size somewhat independently of each other, so that the part assumed a knobbed appearance. Caustic potash applied to one part destroyed it, but the surrounding parts, even although involved in the inflammation produced by the caustic after its subsidence, remained unchanged. Hence, repeated applications of the caustic are necessary. Hebra had previously recorded ten cases of this affection. In one case Hebra removed the knobs by the knife. Under the microscope the hardened tissue appeared to be hypertrophied, thickened cellular tissue.—*Wiener Med. Wochenschr.*, 1870.—*Amer. Journ. Med. Sci.*, April, 1871.

Intestinal Obstruction from a knot on the lower part of the Ileum.—Dr. M. W. Taylor has given an account of a fatal case of intestinal obstruction in which a true knot was found after death on the lower part of the ileum. The patient was a married woman, æt. 40. She lived fourteen days from the commencement of her illness. She had fecal vomiting on the third day, but throughout the attack there was an absence of inflammatory symptoms. After death, about two inches above the cæcum a portion of the ileum, about twenty-two inches in length, was found coiled and involved upon itself so as to form a running knot. The author adds that one instance of true knotting of the bowel was recorded by W. Gruber in 1863. Duchaussoy has related a case very similar to that now given, in his *résumé* of 518 cases of intestinal obstruction. In the fifty-four cases of ileus with autopsies collected by Dr. Hilton Fagg from the records of Guy's Hospital, in seven instances volvulus or twists were found, but in none knots. The author has met with no case of true knotting of the intestine in the British periodical medical literature of the last half century.—*Edin. Med. Journ.*, August, 1871.

CONTRIBUTIONS TO MEDICAL LITERARY HISTORY.

ADVERSARIA MEDICO-PHILOLOGICA.

BY W. A. GREENHILL, M.D. OXON.

PART XI.

(Continued from vol. xlvii, p. 557.)

διάφυσις, a growing or growth between, a partition separating two bodies; also, the space between two or more partitions, an *interstice*. The word occurs in both senses in the Hippocratic Collection.¹ In one passage² the sense both of the text, and also of Galen's Commentary,³ has been understood in several ways. Hippocrates says that at the superior extremity of the tibia there is an epiphysis, and that this epiphysis has a *diaphysis*; by which (says Galen) is meant the ligamento-cartilaginous eminence situated in the middle [of the epiphysis], separating the condyles [of the femur] which rest upon the tibia. Without discussing the different interpretations that have been put upon Galen's words, it will be sufficient to say, with M. Littré, that he probably alludes to the *spinous process* of the tibia, and that this opinion is confirmed by the fact that he expresses himself in very similar language in another passage,⁴ where there is no doubt as to his meaning. In four⁵ other passages where the word occurs in the Hippocratic Collection it may be taken to signify an *interstice*, though in the last of these Dr. Daremberg renders it by "cloison," a *partition*.⁶ Galen applies the word to the *areolæ*, or interstices between the fibres composing cellular tissue;⁷ and also (apparently) to the tendinous intersections (*in-scriptiones tendineæ*) which cross certain muscles, especially the *rectus abdominis*.⁸

διάχριστον, probably a *liniment*,⁹ not a *gargle*, as it was of the consistence of *honey*,¹⁰ and not exclusively (perhaps not even especially) used for the tongue, palate, fauces, &c.¹¹

διά χυλῶν, the name given to a well-known plaster, invented by

¹ See Foësius 'Econ. Hippocr.,' Castelli 'Lex. Med.'

² 'De Fract.,' § 12, tome iii, p. 460, l. 5, ed. Littré.

³ ii, 37, tom. xviii B, p. 475.

⁴ 'De Oss.,' c. 22, tom. ii, p. 774, l. 10, &c.

⁵ 'Mochl.,' § 1, tome iv, p. 342, ll. 11, penult.; p. 344, l. autep.; 'De Arte,' § 10, tome vi, p. 18, l. 5.

⁶ 'Œuvres Choiesies d'Hippocr.,' p. 35.

⁷ 'De Anat. Admin.,' iii, 2; v, 1, tom. ii, p. 350, ll. 12, 15; p. 476, l. 11.

⁸ Ibid., v, 3, 7, p. 496, ll. 8, 10; p. 517, l. 6; 'De Musc. Dissect.,' c. 28, tom. xviii B, p. 994, l. 6.

⁹ Dioscor., 'Mat. Med.,' i, 34, tom. i, p. 47, ed. Sprengel; Paulus Ægin., i, 46, p. 6, ed. Ald.; vol. i, p. 59, ed. Adams.

¹⁰ Paulus Ægin., vii, 14, p. 126 B, ed. Ald.; vol. iii, p. 542, ed. Adams.

¹¹ Aretæus, 'Cur. Acut.,' i, 1, p. 140, l. 1, ed. Adams.

Menecrates in the first century after Christ. His directions for preparing the plaster were put into Greek iambic verse by Damocrates, and the poem has been preserved by Galen.¹ It was called *διὰ χυλῶν*, *with juices*, because certain vegetable substances (fenugreek, linseed and marsh-mallow) were originally used in its composition.²

διαχυσίς, is used by Aretæus³ to signify *relaxation of mind*, and is rendered by Cælius Aurelianus "animi laxatio,"⁴ or "animi laxamentum."⁵

διαχυτικός, is used by Dioscorides,⁶ probably in the sense of *discutient*.

διαχωρέω, *to pass through*, generally as *excrement*, applied to the fæces, but sometimes to the urine;⁷ sometimes also to other matters, as blood,⁸ or food.⁹ Sometimes the word seems to be taken transitively, *to evacuate*;¹⁰ and we also meet with the passive form, *to be evacuated*.¹¹ It is also used as a neuter verb, *to be relaxed*, and is given in the Greek translation¹² of Rhazes 'On the Smallpox' as equivalent to *منطلق muntalik*,¹³ and *لين lain*.

διαχώρημα, *excrement*, especially applied to the fæces¹⁴ (and so synonymous with *ὑποχώρημα*),¹⁵ but probably also used in reference to the urine (see *διαχωρέω*). Honain, in his Arabic translation of the 'Aphorisms,' renders the word by *براز biráz*.¹⁶

διαχώρησις, according to Galen,¹⁷ differs from *ὑποχώρησις*, inasmuch as the latter word is applied solely to excretion from the bowels, whereas *διαχώρησις*, though generally applied to the fæces, is also sometimes used in reference to other evacuations, as *διαχώρησις αἵματος*,¹⁸ evacuation of blood by the bowels.¹⁹ Sometimes the word signifies strictly the process of excreting or evacuating, viz.

¹ 'De Compos. Medic. sec. Gen.' vii, 10, tom. xiii, p. 996, &c.

² It may be noticed that as the *v* in *χυλός* is *long*, the word should properly be pronounced *diachylōn*, not *diachylon*.

³ 'Caus. Chron.' i, 5, p. 75, l. penult., ed. Kühn.

⁴ 'Morb. Chron.' i, 5, iii, 7, pp. 329, 466, ed. Amman.

⁵ Ibid., iii, 8, p. 476.

⁶ 'Mat. Med.' i, 91, iii, 58, tom. i, p. 94, l. 15, p. 405, l. penult., ed. Sprengel.

⁷ Hippocr., 'Aphor.' vii, 67, tome iv, p. 598, ed. Littré.

⁸ 'Coac. Prænot.' § 299, tome v, p. 650.

⁹ 'De Morb.' iv, 44, tome vii, p. 566, l. 13.

¹⁰ Aretæus, 'De Caus. Chron.' i, 15, p. 118, l. 4, ed. Kühn.

¹¹ Id., *ibid.*, p. 117, l. penult.

¹² P. 25, l. 1; p. 256, l. 20, ed. Rob. Steph.

¹³ P. 122, l. 3; p. 160, l. 5, ed. Channing.

¹⁴ Aretæus, 'De Caus. Acut.' ii, 5, p. 44, l. 4, ed. Kühn.

¹⁵ Galen, 'Comment. in Hippocr. Aphor.' vii, 69, tom. xviii A, p. 182, l. 8.

¹⁶ ii, 14, p. 12, l. 3, ed. Calcutta.

¹⁷ 'Comment. in Hippocr. Aphor.' ii, 18, tom. xvii B, p. 489, l. 6.

¹⁸ Hippocr., 'Aphor.' v, 64, tome iv, p. 558, ed. Littré.

¹⁹ In 'Coac. Prænot.' § 387 (393), the old reading was *οὐρων διαχώρησιν*, but Littré (tome v, p. 668), Ermerins (tom. i, p. 86), and Daremberg (p. 280), read *οὐρον*, though Ermerins leaves the Latin translation ('urinarum eductionem') uncorrected.

excretion, or evacuation;¹ sometimes the matter evacuated, viz. the excrement, especially the faeces.² It is rendered by Honain براز *biráz*, and خروج *churúj*.³

διαχωρητικός, *calculated to promote evacuation* (especially from the bowels), *laxative*.⁴ Hippocrates⁵ uses the comparative διαχωρητικώτερος.

διδυμαία, *didymæa*, the name given by Andreas (probably in the third century B.C.) to a medicinal preparation, the composition of which is preserved by Asclepiades.⁶ It is in vain to attempt to explain the meaning of the name; it can only be stated that it is certainly not derived from the following word, but may probably be connected with *Didyma* or *Didymi* in Miletus, whence comes the adjective Διδυμαῖος, *Didymæan*.

διδυμος, properly an adjective, *double*; but in medical writers generally used substantively⁷ as a technical term. In very early times διδυμοί,⁸ or more commonly διδύμα,⁹ signified *twins*; the former word was applied to the *testicles* by Hierophilus¹⁰ as early as the fourth or third century B.C., and the name seems to have been generally adopted, and used indiscriminately with ὄρχις,¹¹ except that it was not so often used in the singular number,¹² and was considered a more polite word than the other.¹³ The word was also applied to the ovary in the female, the part analogous to the testicle in the male.¹⁴ The diminutive form, found probably only in the

¹ Hippocr., 'Aphor.,' ii, 18, tome iv, p. 474, ed. Littré. Aretæus, 'De Caus. Chron.,' i, 13, p. 109, l. 10, ed. Kühn.

² Hippocr., 'De Rat. Vict. in Act.,' § 9, tome ii, p. 438, l. 11, ed. Littré, where Galen, in his 'Commentary' (tom. xv, p. 824, l. antep.), used διαχώρημα, as a synonym.

³ 'Aphor.,' iv, 47; ii, 18, pp. 36, 1; 12 ult.

⁴ Aretæus, 'Cur. Acut.,' i, 2; ii, 5, p. 202, l. 8; p. 273, l. ult., ed. Kühn.

⁵ 'De Rat. Vict. in Act.,' § 14, tome ii, p. 332, l. 7, ed. Littré.

⁶ Galen, 'De Compos. Medic. sec. Loc.,' x, 2, tom. xiii, p. 346, l. 7.

⁷ Galen uses it as an adjective in the expression διδυμοί ἄνθρωποι, *men that are twins*, 'De Anat. Admin.,' iii, 5, tom. ii, p. 384, l. 8.

⁸ Homer, 'Il.,' xxiii, 641; Herod., 'Hist.,' v, 41, § 4.

⁹ 'Herod.,' 'Hist.,' vi, 52, § 2; Hippocr., 'Aphor.,' v, 38, tome iv, p. 544, ed. Littré.

¹⁰ Galen, 'De Usu Part.,' xiv, 11, tom. iv, p. 193, l. 6.

¹¹ Rufus Ephes., 'De Appell. Part. Corp. Hum.,' p. 31, l. 20, ed. Clinch.; Pseudo-Galen, 'Introd.,' c. 10, tom. xiv, p. 706, l. 8; Meletius, 'De Corp. Hum. Fabr.,' c. 26, p. 113, l. 7, ed. Cramer.

¹² The note to Theophilus, 'De Corp. Hum. Fabr.,' v, 37, § 4, p. 264, l. 13, ed. Oxon., requires some modification. The passage in which Galen used the word in the singular is referred to ('De Usu Part.,' xiv, 11, tom. iv, p. 193, l. 6), and it is stated that "alibi *fere semper* in plurali occurrit." When I wrote this I had not noticed that in Paulus Aegineta the word is found at least six times in the singular (viz. iii, 54, p. 50 B, l. 34, ed. Ald.; vi, 61, 68, p. 258, l. 4; p. 260, ll. 1, 4, 8; p. 288, l. antep., ed. Briau), so that it is probably less uncommon than I then supposed.

¹³ Galen, 'De Anat. Admin.,' ix, 5, tom. ii, p. 729, l. 8.

¹⁴ Galen, 'De Usu Part.,' xiv, 11, tom. iv, p. 193, l. 6; Theophilus, 'De Corp. Hum. Fabr.,' v, 37, § 4, p. 264, l. 13, ed. Oxon.

plural, διδύμια, is applied almost always¹ to two of the *corpora quadragemina* in the brain, which still bear the name of *testes*.² Albucasis, in his translation of the sixth book of Paulus Ægineta, renders διδυμος by بَيْضَة *baidat*, and διδυμοι by اُنْثِيَان *unthayán* (dual), and خَصَا *chasá* (plural), and بَيْضَاتَان *baidatán* (dual).³

διδυμοτοκίω, to bear twins.⁴

διδυμοτοκία, a bearing of twins.⁵

διδυμοτόκος, bearing twins.⁶

Reclamation from Dr. Matthews Duncan.

To the Editor of the 'British and Foreign Medico-Chirurgical Review.'

SIR,—It is with reluctance that I request your attention to an act of injustice to me which your obstetrical reporter has done—no doubt unintentionally.

Some years ago a similar injury was done me in connection with my paper "On Inversion of the Uterus." Having represented the matter to Dr. Barnes, I was pleased to find that in the next appearing number of your Journal the injury was courteously undone by a brief statement. (See page 396 of my "Researches in Obstetrics.")

On the occasion referred to I was erroneously represented as being forestalled in my theory by an Italian. On the present occasion I am represented as "*basing on*" a German; and I certainly do not in my paper acknowledge indebtedness to him as I ought to have done had I based on him. A serious moral delinquency is here implied against me, and it has not the slightest foundation. It is a gratuitous and unjust assumption by Dr. Barnes, and I am astonished at his conduct, because I am sure I have never given him or any one ground even to suspect me of such a thing as "*basing*" on another without acknowledging it.

So far is Barnes's implied accusation from the truth that the paper was written nearly as it now stands, and the drawings set agoing, before I knew of the German author's paper, and before the paper was in Edinburgh. So far was I from "*basing on*" Lemser, the German alluded to, that I have for many years publicly taught the views expounded in my paper on the expulsion of the placenta. Lemser's paper is on the separation of the placenta, and appeared in 1865, that is, long after I had been teaching all that is in my paper now reported

¹ Paulus Ægineta uses the word with strict propriety when speaking of the testicles of a little child (vi, 68, p. 288, l. 16, ed. Brian).

² Galen, 'De Anat. Admin.,' ix, 5, tom. ii, p. 729, l. 7; 'De Usu Part.,' viii, 14, tom. iii, p. 678, ll. 3, 5, copied by Oribasius, 'Coll. Medic.,' xxiv, 1, § 24, tome iv, p. 280, ll. 9, 10, ed. Daremb.

³ 'De Chirurg.,' ii, 69, p. 313, ll. 12, 15, 16, penult., ed. Channing.

⁴ Aristotle, 'De Gener. Anim.,' iv, 6, p. 135, l. 20, ed. Bekker.

⁵ Aristotle, *ibid.*, iv, 4, p. 129, l. 24; Theophilus, 'De Corp. Hum. Fabr.,' v, 31, § 10, p. 239, l. 9, ed. Oxon.

⁶ Aristotle, 'Hist. Anim.,' vi, 19, p. 182, l. 8.

on by Barnes. Besides, it was impossible for me to base my observations or remarks on those of Lemser, which are on the separation not on the expulsion of the placenta, a quite different subject.

I beg you to publish this letter, or take other means of repairing the injury of which I complain. I am, sir, yours faithfully,

J. MATTHEWS DUNCAN.

Notes from Dr. Barnes, in reply to Dr. J. Matthews Duncan.

The Editor has placed in my hands a rather warm reclamation from Dr. Matthews Duncan, complaining that in the abstract of his memoir on the "Mechanism of the Expulsion of the Placenta" (see our Report on Obstetrics, July, 1871), it is implied that he has been guilty of the "serious moral delinquency" of "basing his views upon Lemser" without acknowledgment. The memoir of Dr. Duncan was published in the 'Edinburgh Medical Journal' for April, 1871. Lemser's "Memoir on the Physiological Separation of the Placenta" was published in 1865. Dr. Duncan says in his memoir that "his remarks are remarkably corroborated" by this memoir of Dr. Lemser. But he further says in his reclamation that "he has for many years publicly taught the views expounded in his memoir, and that it was impossible for him to base his observations on remarks on those of Lemser, which are on the separation, not on the expulsion of the placenta, a quite different subject." Lemser shows that the placenta on detachment *presents* at the os uteri by its edge; Dr. Duncan shows that it is *expelled* by its edge, rolled up longitudinally, and not turned inside out, as it is found to be when it is extracted by pulling upon the cord. It appears, then, that the fundamental fact, the necessary antecedent to Dr. Duncan's discovery, is contained in Dr. Lemser's memoir. And I must be excused for pleading that I could not be expected to know what Dr. Matthews Duncan had been teaching for years, and that I could only compare published matter and dates. I might also plead that I myself have for many years taught a similar doctrine of the mechanism of the detachment and expulsion of the placenta, without, however, reflecting that I had made a discovery deserving special record.

I may, in conclusion, be permitted to state that the report referred to is the last which will be contributed by me. During more than fifteen years that I have supplied the reports on obstetrics, it has been my good fortune to receive many expressions of approval and thanks for the manner in which a duty, delicate and arduous, has been performed, from the successive editors of this Journal, and from men who cultivate obstetrics in different parts of the world. During that time not a single reclamation imputing unfairness has come to me from any one excepting Dr. Matthews Duncan. In making my bow I could have wished that this solitary exception did not exist. As this cannot be, I must be content with the consciousness that Dr. Matthews Duncan is no exception to the rule of impartiality and friendly recognition with which I have always tried to welcome the contributions of my fellow-workers in the field of obstetric medicine.

ROBERT BARNES.

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Essay on Growths in the Larynx, with Reports, and an Analysis of 100 consecutive cases treated. By Morell Mackenzie, M.D., &c. With numerous Illustrations. London, Churchills. 1871. pp. 263.

On the Physiological Effects of Severe and Protracted Muscular Exercise; with especial reference to its Influence upon the Excretion of Nitrogen. By Austin Flint, jun., M.D., &c. New York, Appleton and Co. 1871. pp. 91.

A Treatise on Diseases of the Nervous System. By W. A. Hammond, M.D., &c. With 45 Illustrations. New York, Appleton and Co. 1871. pp. 754.

The Medical Jurisprudence of Insanity. By J. H. Balfour Browne. London, Churchills. 1871. pp. 341.

Braithwaite's Retrospect of Medicine, Vol. LXIII. January to June, 1871. London, Simpkin, Marshall, and Co.

The Half-yearly Abstract of the Medical Sciences, Vol. LIII. January to June, 1871. London, Churchills.

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General Index to the First Fifty-three Volumes of the Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. London, Longmans. 1871. pp. 355.

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A Manual of the Laws affecting Medical Men. By Robert G. Glenn, LL.B., Barrister-at-Law. London, Churchills. 1871. pp. 460.

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Twenty-fifth Report of the Commissioners in Lunacy to the Lord Chancellor. Ordered by the House of Commons to be printed, July, 1871.

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St. Moritz as a Health Resort, and the Baths of Bormio. By R. Whitfield Hewlett, M.D., &c.

On some Forms of Pneumonia. By Robert Farquharson, M.D.

The Dental Profession. By a Dental Surgeon.

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